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ABSTRACT

The contributions of various disciplines to the field of higher education and research studies are discussed in 10 seminar papers, which also review Burton R. Clark's work, "The Higher Education System: Academic Organization in Cross-National Perspective." Titles and authors are as follows: "Internal and External Regulatives in Research and Higher Educational Systems" (Aant Elzinga); "Research Organization and Research Policy in Sweden after the Second World War: A Discursive Essay" (Thorsten Nybom); "Disciplinary Discourse" (Tony Becher); "The Ethos of Higher Education Systems: A View from Anthropology" (Tomas Gerholm); "Integration and Higher Education" (Sven-Eric Liedman); "Higher Education and Research in an Educational Perspective" (Lennart F. Svensson); "Sociology and Organised Knowledge: Changing Perspectives in the Sociology of Science" (Katrin Fridjonsdottir); "The Higher Education System in a Time Perspective" (Rolf Ohlsson); "Are Some Disciplines More Relevant Than Others?" (Rune Premfors); and "Extending 'The Higher Education System'" (Burton R. Clark). (SW)

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UNIVERSITY OF STOCKHOLM

Group for the Study of
Higher Education and Research Policy.

REPORT NO. 37 FEBRUARY 1987

DISCIPLINARY PERSPECTIVES ON HIGHER EDUCATION AND RESEARCH

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INTRODUCTORY NOTE

This Report in the GSHR series consists of papers presented at a recent International Seminar, "Disciplinary Perspectives on Higher Education and Research" (Lidingö, Sweden, September 2-3 1986). The Seminar was arranged by the Research on Higher Education Program, an autonomous research program sponsored by the National Board of Universities and Colleges. (A list of participants is appended.)

The invitation sent out by the convenor asked paper authors - who had been selected to represent a wide range of relevant disciplines - to survey research and develop ideas which illustrate the possible contributions of their specialities to the field of higher education and research studies. In addition, contributions should contain "a critical review" of Burton R. Clark's highly praised work, The Higher Education System: Academic Organization in Cross-National Perspective (Berkeley, Los Angeles, London: University of California Press, 1983; paperback edn, 1986).

The result is a highly readable set of papers. They provide a wealth of information on the actual and potential contributions of various disciplines to the advancement of knowledge in the field. And they formulate many constructive criticisms of Clark's magnum opus - criticisms which are skilfully answered by Clark in his paper "Extending The Higher Education System".

All in all, we consider it a priviledge to publish the Seminar papers as a Report in the GSHR series. The papers have been revised by the authors and slightly edited by me. Thanks are due to Eskil Björklund of the Research on Higher Education Program for convening such a stimulating meeting, to the authors of the papers, to the discussants and other participants at the Seminar, and to Anne-Marie Eklund of the GSHR for her skilful handling of the word processor.

Rune Premfors

Chairman, GSHR

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Introduction

Burton Clark's book The Higher Education System. Academic Organization in Cross-National Perspective is very rich in content. There are many different strands that would be interesting to pursue. However in this paper I shall restrict myself to only take up a few that are most natural from the vantage point of the philosophy and theory of science as we conceive it.

I am working in a tradition where the theory of science is seen as an empirically-based discipline living in symbiotic interplay with other disciplinary specialities under the umbrella we have come to term "Research on Research" (or in Swedish - "forskning om forskning"; the Swedish terminology is more natural and adequate than the English translation). Other scholars under the same umbrella come from history of science, political science, psychology, economics, sociology etc., to focus upon problems of historiography of science, science policy studies, psychology of science, sociology of science and knowledge generally, the economics and organization of science, R&D management, etc. Two years ago we produced an anthology in Swedish under the title "Research on Research - or the art of depicting an elephant" (Bärmark, 1984). The "elephant" in question is "science", and the subtitle alludes to an Indian tale of ten blind men who approached an elephant. One took hold of the elephant's tail, claiming he had found a string. A second got the animal's trunk in his hands and claimed with equal conviction that his colleague was wrong - no "this is a large snake". A third blind man bumped into one of the legs and putting his arms around it argued that what they had really found was a tree (he was spanning his arms around what he thought was a tree-trunk), and so on. Each of the men totalized his own findings and perspective, and they got into arguments with each other, regarding what it really was they were exploring.

So it is with science too. There are many disciplinary entries into the study of research as a process, science and higher education as a system, knowledge as power, and so on. Each discipline has its own preferred set of indicators of the "empirical", and within each discipline too there are schools and controversies regarding the interpretation of findings. I make this comment at the outset, because I think it is relevant to the present task of commenting on Burton Clark's very perceptive and enjoyable book, in which he also has some important policy recommendations. I can also say that, even if my own disciplinary and ideological points of departure differ from those of the author, in many respects I arrive at similar conclusions as to policy for systems of higher education and research. This will be evident to those who are familiar with some of my writings concerning the university research system and my critique of the Swedish reality in particular (Wittrock & Elzinga, 1985; Elzinga, 1969 and 1982 for example). There is no point in repeating here what has already been stated elsewhere. More interesting is to somewhat delineate differences in perspective and also to see how we can come further in the discussion of the very important issues at hand raised in the author's

book which is here under review as a point of departure for comparative cross-national dialogue.

The issues I should like to bring to the fore are the following: what are the dynamic processes in higher educational systems and how shall we understand them; are there boundary conditions to differentiation and academic drift, and to what extent may these be cognitive in nature; should we argue for the freedom and integrity of science in moral terms (referring for example to humanism and the critical function of universities in our civilization), or should a case be made on the basis of pragmatic arguments referring to the fact that scientific knowledge production and higher education functions "better" if there is variety and pluralism (Clark). A third possibility is to argue that the very nature of science as knowledge producing social institution requires a position of relative autonomy which can be backed up by rational epistemological criteria that require a subordination of equity to excellence. In my own view this third standpoint is only possible if we unpack the equation "equity and excellence" so that it becomes clear that one is dealing with a number of different dimensions, that in simplified form and when juxtaposed, become incommensurable.

Unfortunately, just as in the story with the ten blind men, there is often a strong element of incommensurability built into the meeting of different disciplinary perspectives, especially when the field of analysis involves something as complex as "higher educational systems" in a large number of different national and local settings. In one sense we are all participants in a local or national higher educational culture (or cultures), and in our travels in different countries where we visit colleagues and learn about the social and institutional arrangements that set bounds for their research and higher educational activities, we all are at once "experts" and amateurs with respect to the issues under discussion. This complicates the situation even more.

Having said all this I will now dig into the various themes I have mentioned, with a separate sub-heading for each. In a final section I shall introduce two further topics which may be used as lithmus paper to test cross-national comparisons and discuss similaritites and differences at the level so interestingly presented in the "Triangle of Coordination" on page 143 in Burton Clark's book. The additional topics I shall introduce pertain to the emergence of new disciplines and secondly national styles in "foresighting" science and technology for development. In regard to the first topic I will base myself on some of our findings in studying the emergence of nursing care research as a new academic specialty in Sweden (compared to Britain and North America). Since the publication of the monograph Omvårdnad som vetenskap och ideologi (Bärmark, Elzinga & Wallén, 1983) we have considerably widened our approach to consider a number of other newly emerging disciplines in Sweden.

In regard to the second topic - "foresight" - I shall make reference to the recent publication of John Irvine & Ben Martin Foresight in Science. Picking the Winners (1984) which deals with national styles in targetting basic research. In addition I will refer to some of my own experience in working operationally in this field for one year at the Science Council of Canada where we set up a unit for Emerging Science and Technology (EST).

Dynamic forces

Differentiation is one process whereby new disciplines and specialties emerge and are established. Internally this may be seen as the result of new disciplines splitting off from older ones, new areas of research being opened up. The driving forces in this process viewed internalistically may be understood either as social and socio-psychological or as "cognitive", or both. Sociologists tend to focus upon the social determinants. Scholars studying systems of higher education may do likewise, and they may even attach to the sociologists interest-group theories. Philosophers of science for their part like to focus on the cognitive dimensions of knowledge production, and therefore they see differentiation as a taxonomic process of new boundary-lines being drawn on our knowledge maps. This either as a result of new areas or aspects of reality coming to light, or old areas being viewed with new eyes (paradigm shift), or in some other way suggested by one's particular theory of knowledge.

Externally the emergence of new disciplines may be viewed as the consequence of socio-economic demands or political decisions, or a combination of these. An example in Sweden is the decision expressed in the university reform which brought all lines of higher vocational education into the national university system, thus raising the question of the ways and means of their "research attachment"; that is to say, the vocational lines should have a research connection enabling graduates to go on to do research work within the domain of their own professional competency. We have already "nursing care research", research on social work, research around physical therapy (rehabilitation research), and in the future we shall probably have "police research" within the confines of higher education in our national Police Colleges.

From an externalist perspective some historians of science would say that the driving forces here were socio-economic and political forces in society at large. Sociologists and some philosophers of higher education might agree with Habermas and Gernot Böhme in Germany or Gunnar Bergendahl in Sweden that what we see here is a "scientification" of traditional practitioner vocations which have until now managed quite well without a research connection or "basis" in science. This process may then in turn be explained in neo-Weberian terms as a "modernization" and rationalization of further areas of present day society. The three persons cited above are all critical, and in different ways they warn for the loss of important realms of life-world knowledge and the disappropriation of power from the hands of traditional practitioners and into the hands of academicized professionals. Another type of explanation refers to the professionalization of certain, traditional practitioner groups like nurses, physical therapists and social workers. In this case the driving force is seen to be the strivances of these groups to increase their professional status and competing for power in the health care system by developing Ph D's amongst their own cadres. This is a perspective that may be given a sociological internalist turn by calling on interest-group theory.

One symptomal reading of Burton Clark's book brings me to interpret his view of dynamic forces along the lines of the interest-group explanation. Indeed he says explicitly in his introductory chapter that when he seeks to explain changes in the structure of work, belief and authority, "the interest-group struggle, largely internal, is at the heart of the dynamics of the system". But then he adds: "Seen in the large, the struggle is shaped by what we might call the underlying

'academic forces of production', forces that steadily produce contradictions and, at the same time, aid certain interests more than others. Perhaps most basic is a steady process of differentiation that seemingly undergirds both the internal and external forces of change" (op.cit. p. 8).

Two comments are in order. From the point of view of the externalist historian of science or the Habermasian sociologist of knowledge, what Clark refers to as "external" is really "internal". On the other hand, and this is my second point - from the angle of much of rationalist (including Popperian) philosophy of science, what Clark sees as "internal" is really external, while the really "internal" dynamics of differentiation are ignored by Clark (although less so in Clark, 1984).

I shall come back to this second point in the next section, where the process of "differentiation" will be taken up further. But before that let me refer to a useful schematization used by Stuart Blume to distinguish internal and external driving forces in the higher educational system.

Blume cites Clark's book and how the latter has drawn attention to some of the implications for universities of their partial dependency in their structures upon knowledge or discipline structure (Blume, 1985). Blume agrees that one may argue that the structures of universities now largely reflect knowledge structures (which may be functional for advancement of knowledge), and that this tends to force education into a specialized form. However he does not question Clark's interpretation of this postulate in group-interest terms, according to which we must understand the organisational replication of knowledge structures micropolitically and socially and not primarily as reflective of basic epistemic resilliance at the level of cognitive structures. On the other hand Clark does have a point when he indicates that disciplines do not necessarily represent the structuring of knowledge best suited to preparation of the student for an occupational role other than that of the research scientist (cf Blume, p. 144). As I have already indicated above one of the driving forces at work in the restructuring of knowledge structures today may well be strivances within professions of nursing, physical therapists, social workers and the like. But these strivances may in turn be linked to external factors like increasing complexity of administrative routines in modern hospitals, social welfare legislation and the emergence of new health care technologies (high tech hospitals), all of which require new technical, administrative and other competencies amongst health care and social workers.

Blume's suggestion is that there are four fundamentally different processes at work in bringing about changes in the structures of systems of university higher education. He refers to these as the processes of differentiation, extension, external stimulation, and professionalisation. Differentiation consists in the branching off of new specialties, a process which may be understood both sociologically and epistemologically. Extension is the process whereby one seeks to extend the scope of science to cover areas of experience, phenomena, where previously there was no science. In one sense this may be associated with what I have already referred to as the process of scientification of traditional realms of life-world practitioner knowledge. In contrast to Blume, who sees both differentiation and extension as <u>internal</u> dynamical factors, I have indicated that scientification may also be viewed from an externalist perspective as the workings of a

more general societal process (rationalization or the development of technical reason, modernization, or some such process).

In Blume's schematization, both external stimulation and professionalization are external dynamic factors of change. External stimulation refers for example to the development of technological or applied sciences in response to essentially external needs or demands. In the account Boris Hessen gives of Newtonian science, or Bernal in his Science in History we meet a host of examples of this type. In the Swedish discussion Sven-Eric Liedman, historian of ideas, has suggested that we should distinguish between three types of utilization of scientific knowledge: productive (in the economic sense), administrative and ideological. The natural and technological sciences fall to a large extent into the first category, the social sciences into the second, while arts and humanities have ideological uses. The various use-contexts may be seen as instances of external stimulation, and Blume's concept may thus be widened to include even the non-technological sciences and varieties of learning. Indeed it is not unusual to find new fields of investigation crystalizing around not only a technical concept (biotechnology, advanced industrial materials), but also around new administrative practices, welfare needs or an ideological discourse in society. In the latter case a field in the humanities serves legitimatory purposes. Professionalisation finally may be studied in terms of professionalisation strategies, where the university connection becomes an important element in upgrading knowledge and providing competencies with a scientific base, as well as giving the profession greater clout vis a vis other professional groups (e.g. nurses against doctors in the hospitals or in the health-care system more generally).

Resistance to change and obstacles to attempted reorganization of curricula or institutional arrangements within universities may also be analyzed in terms of the internal/external distinction provided here in Blume's terminology. With the widening of his concept of external stimulation to cover even administrative and ideological phenomena it then becomes pertinent to ask how external administrative structures and ideological "apparatuses" (Poulantzas) facilitate the development of certain knowledge fields and not others.

It is not possible here to go into more detail or to do justice to some of these complications regarding the interplay of social and cognitive structures of the sciences and the underlying dynamics of internal and external factors of various kinds. One interesting attempt in this direction however is the strong programme in the sociology of science, another is the Finalization thesis developed in Germany during the 1970's (Schäfer, 1983; cf also Böhme, 1979).

More about differentiation

Clark defines differentiation as "driven in the immediate setting by the rearrangement of interests; interest is basically divided between those already vested and those seeking to become vested; the outcomes of interest group struggles are determined by relative power, and power is rooted in respective legitimacies" (p. 218). Differentiation and specialization here is not tied to the internal dynamics of cognitive differentiation in sets of epistemological taxonomies (disciplines) which bear epistemic mapping relations to domains in the world of objective reality. Rather it seems primarily to be conceptualized here as a social mechanism of distribution of power amongst elites, as a gambit used by interest groups to carve out a niche for themselves:

"differentiation is a means of group protection", Clark also says, quoting Durkheim (p. 221).

The problem is: if pressed far enough this standpoint will lead to voluntarism and relativization as far as knowledge validating criteria and processes are concerned. In my own view - and I am sure also in the author's - differentiation is certainly more than just the result of a social and micro-political process in the world of higher education. But since the author fails to bring out what this "more" might be (his section on "knowledge" is weak here), he consistently plays down the relative autonomy of the cognitive dimension.

In the Swedish discussion Göran Wallén, in polemics with advocates of the strong programme in the sociology of science (and also against similar relativizing standpoints in ethnomethodological approaches to the social studies of science and technology), has argued how steering and structuring factors sometimes dominate clearly over social and institutional or interest group factors in the dynamics of research. His argument is based on case studies of the Swedish Coniferous Forest Ecology Project which developed through several distinct phases of crisis and reorganization during a ten year period in the 1970's. The SWECON project as it is called, employed about 80 researchers in various disciplines, and it had a Steering Group responsible for co-ordination and research strategy. Wallén has found that in a certain conjuncture in the history of this project (which may be understood as a large network institution) theoretical and methodological problems became determinants for the way in which social and organisational structures were built up within the project, and not vice versa (Bärmark & Wallén).

Sven-Eric Liedman in a monograph on Scientific Specialization points out how specialization within scientific fields is not identical with division of labour. The latter is a process in society, whereas the former has to do with a certain degree of specialization of competence (a repertoire of specialist knowledge and praxis). Such division may give a measure - in society - of complexity and in how far a specific job requires knowledge and know-how above and beyond what the average citizen receives through his schooling. The former on the other hand gives a measure of the degree of differentiation of the scientific knowledge system internally and to what extent a specialist in a certain field requires specialist knowledge beyond a generalist training in science. Thus Liedman distinguishes between the social specialization of science and the internal specialization of science, and reminds us that these are two quite different things (Liedman, 1977 p. 7 ff). On this basis he develops a model in which external stimulation and professionalization do not work <u>directly</u> on the knowledge structure of science and higher education, but only indirectly and by mediation through the structures formed on the basis of internal specialization. Thus he concludes that "driving forces behind specialization cannot be sought in immediate sociological and psychological - here first and foremost in terms of the sociology of science and the psychology of research - relationships" (ibid., p. 4). This does not mean that such relationships are unimportant. On the contrary. What it does mean however is that if one wants to explain significant changes - historical changes as it were - in knowledge structures and social structures in science and higher education, it is of little use to come with factors like competition, status or interest-group rivalry. The effect of such factors proceeds only indirectly in mediated form, as processes in another dimension.

Knowledge taxonomies organized in disciplinary institutions certainly do have a social epistemological dimension. However this does not mean that they are socially and institutionally maleable in whatever direction one likes. What is interesting for the theory of science is how epistemological resilliancy and stability expresses itself, and what it signifies.

Academic drift

The countervailing tendency to differentiation referred to by the author in his book is "academic drift". It is also largely defined in social interest-group and power terms. Academic drift is the tendency for educational systems to converge upon one specific organisational form, that of the university system. Public four year colleges strive towards university status; normal schools become teachers colleges, and eventually they may go on to become state universities. Second best educational institutions pattern themselves on their richer and more distinguished cousins in the Ivy League of first class universities, and so on. The convergence process is said to hinge on a quest for distinctiveness, which may in turn be analyzed in terms of a dynamics of interest-group struggles.

As before, I have difficulty with these statements if they are taken to be about direct and unmediated relationships between socio-psychological factors and cognitive structures in the university system. If on the other hand they assume some type of mediating play, this is something that is not brought out or developed in the text. Therefore I shall assume that we are dealing with socio-psychological or in some cases socio-economic causality. My criticism then is basically that the author falls into a species of reductionism. Voluntarism has already been mentioned; socio-economic reductionism is another possibility.

The question I want to raise, also here, concerns the cognitive dimension of knowledge taxonomies seen from the point of view of social epistemology.

What happens at the level of knowledge structures when a convergence process (as subsumed under the term "academic drift") takes place? Is there also a convergence in some cases towards the disciplinary and interdisciplinary terrain of cognitive maps which are found at the first class universities, or can institutions in quest of distinction define their pursuit for distinctive niches in purely non-cognitive terms? Can they ignore the fact that there exists a reality of internal scientific specialization which requires specific hard core scientific competence in order to make it? As you will have guessed, my own answer is no. The interesting thing here would be to investigate stabilities and instabilities at the level of "symbolic generalizations", to use Kuhn's term referring to the uppermost tier in what constitutes the "paradigm". Reference may also be made to the next tier - necessary methods and scientific instrumentation which has to be run by people.

It is clear that there are strong social and micro-political forces that structure the relevance profiles and contents (curricula) of higher educational systems. Arthur Levine's spectrum of strategies of innovation may be used to take the discussion one step further, if we interpret these strategies as saying something also about the cognitive stabilities and dissonances that set boundaries for what is

possible and not possible under the following headings (cf. Clark, p 225):

> diffusion - the ways in which the innovative part is spread through the host organization (and there achieves hegemony);
>
> enclaving - where the innovative unit maintains its own ways but in an isolated position in the earlier organization (where the traditional "ways" maintain social and cognitive hegemony) - this is often seen in Sweden, e.g. in the case of the Center for Interdisciplinary Studies of the Human Condition (Centrum för tvärvetenskap) at the University of Gothenburg.
>
> resocialization - where the innovators fall back in line with traditional values and practices (in traditional universities this can also be seen as a "disciplinary strategy" - falling back in line with an existing discipline, cf. Elzinga, 1985 p. 212).
>
> termination - when the innovation is eliminated (by economic retrition or other means).

In each of the foregoing types of strategies there must be some form of mediation between the needs and interests of the actors concerned and the social and cognitive structures to be reshaped. Perhaps it might be useful, as in the German and Dutch discussions, to identify social and cognitive factors of resistance and receptivity. As Arie Rip has discussed in his review of some of the relevant literature, resistance to innovation and changes of policy follow from both social and cognitive factors (Rip, 1981, e.g. p. 305). Resistance to reallignments may for example be contingent on a field of research being in a state of immature theoretical development, in which case there is a cognitive deficiency. In other words, in order too meet professional demands or interest group strivances, or some form of external stimulation, the field must first develop a more mature theory. Lacking this, efforts to translate practical needs or problems into scientific research problems to be tackled by specialists in the field, will not succeed - not until the theory is developed, and useful matching between external needs and internal competencies can be established. Cognitive dissonance occurs when the requirements of diffusing an innovation into a new area brings to light such a mismatch. This contradiction will then either spur on theoretical development and a strategy to develop the necessary cognitive competence, or else the opposite strategy of setting up a barrier of resistance and carrying on as usual.

In conclusion then, I think there is some evidence that also academic drift and boundary conditions for innovation strategies may depend upon peculiar dynamics of cognitive structures which cannot be explained away in sociological, psychological or group interest terms in an account of why things change in the way they do in the world of research and higher learning.

Epistemic drift

I now wish to say something about another drift phenomenon, which I find equally important compared to academic drift. It is a tendency which works in the opposite direction to academic drift. It is the tendency whereby knowledge structures and social structures in the university system tend to become reorganized into disfunctional patterns when research communities come under strong external pressures

of relevance and accountability. It is the phenomenon I have discussed in my article "Research, bureau.pa cracy and the drift of epistemic criteria" (Elzinga, 1985). I call it "epistemic drift".

Advocates of the Finalization thesis have held that when theoretical fields are mature science becomes ripe for external stimulation and political direction. Cognitive dissonances cease to exist and the externalizatio· of epistemic norms is the natural thing. Resistance to external dire ion can then only be understood as a manifestation of social resistance, which may be explained in terms of the interest group thesis.

Finalization theorists have been criticized for ignoring the fact that scientific cultures are very heterogeneous. What is possible in physics is not possible in linguistics or ethnology. Also the case studies that have been carried out have not succeeded in demonstrating how social factors "influence" "truth" in fields of knowledge. Indeed this very thesis has helped fan the flames of relativism in the debate concerning relativism and objectivism. Others have maintained that objectivism is essentially a manifestation of Cartesian anxiety, the fear of trembling, living with knowledge without foundations (Bernstein, 1983).

More serious I think is the implicit notion in the Finalization thesis that in mature science we can forget about the traditional criteria of excellence and freedom of inquiry. The principle of autonomy would seem to become obsolete as an epistemological principle - it can only serve as a principle for defending elitism and subordinating equity to excellence, non-scientists or laymen under the rule of experts.

In my view this conclusion cannot be drawn from the Finalization studies, nor from other social studies of science and higher education. Even if we were to agree that certain fields of research become mature in the sense that no further theoretical developments are needed or possible, it does not follow that the researchers' right and responsibility ceases regarding the need to translate practitioner problems into research problems. This is not an automatic process. Also the researchers' responsibility for maintaining high standards and choosing methodologies or basic concepts, accepting or rejecting hypotheses, or publishing their results for scrutiny in the scientific community, all this does not cease to hold. The principle of autonomy cannot be ditched; on the contrary, strong relevance and accountability pressures (external pressures), must be met with increased consciuosness concerning the internalist mechanisms for quality control in the knowledge process.

My epistemic drift thesis involves the following postulate: increased political determination and relevance pressure on the sciences, can in the absence of a strong research policy consciousness on the part of disciplinary researchers lead to a confusion of external and internal epistemic criteria. Criteria of social relevance become dominant and quality control criteria (validation, verification or some such) lose their tenacity. A shift of focus occurs from internal to external norms - in short "epistemic drift" in the regulatives encoded in epistemic criteria.

Epistemic drift therefore can be understood as the shift from a traditional system of quality control in knowledge structures, based on peer review and disciplinary reputational systems to externally based regulatives and controls. The latter may crystallize arround sectoral functions of knowledge utilization (housing, defence, development aid,

health care, social problems, etc.) as defined by the Ministerial structure within the state apparatus, or it may be around social and political movements (environment, women, black studies, etc.), or again around other external lines of "stimulation" pressures.

It is evident then that epistemic drift today is not alwalys and necessarily linked to the evolution of bureaucratic and state dirigist penetration into the institutions of research and higher education. The fact that these aspects have been emphasized in my previous studies is a reflection of the Swedish reality of an earlier phase, the 1970's when sectorialization of science was the primary issue on the national policy agenda. Today private investment in university affairs is increasing, and commercialization of research and higher education is beginning to overshadow the earlier issue of state dirigism and corporativism at the national level. A prime policy issue today is defined in terms of local science and technology parks, industrial parks, little "Silicon valleys" cropping up around Swedish universities. Higher education and research in the field of economics and business administration is also undergoing restructuring at the University of Gothenburg, in response to clearly defined commercial stakeholder interests in the region.

At an international conference three years ago, organized by the Swedish Council for Planning and Coordination of Research (FRN) through its Committee for Future Oriented Research (SALFO), the issue was perceptively defined as "Science as a Commodity. Threat to the Open Community of Scholars". Quoting from Michael Gibbon's Introduction to the book that came out of this conference, it appears that the perception of a "commoditization" arises out of "the tension between the way science (or knowledge) is <u>used</u> in our societies and the way in which it is supposed to be <u>generated</u>. The tension arises because it is not clear whether the knowledge that is generated is being used properly or whether if it were to generated properly it would be useable" (Gibbons & Wittrock, 1985, p. x). Gibbons also quotes Rothblatt who like Blume refers to two distinct sets of pressures operating in the scientific community, one from the outside and one inside. These pressures operate also in the definition of what science is and should be. "'The first step of pressures consist of demands for a wide range of scientific services for government, industry and the military... The second set derives from the internal constitution of science, from its cultural or value system and from the institutions that scientists have built or have cooperated in building in order to maximise the conditions under which their work is performed'. Here, the operation of these two pressures, an external pressure attempting to mould science to social need and an internal pressure aimed at acquiring social acceptance for scientific knowledge, resulted in the professionalization of science which has given the scientific enterprise the shape it has today" (ibid., xiv).

It is this shape that is being challenged today, from two sides from without - via a continuing pressure of mandating science in line with political and bureaucratic decision-making institutions, and via the pressure of market forces at a time when emerging technology clusters like microelectronics, biotechnology and advanced industrial materials have to be based on strategic reserves of basic research. In this latter dimension the role of science as a weapon in the struggle between industrial nations over international markets has also become more acute. This aspect will be taken up in my discussion of foresight (see below). What is important at this point is the complex nature of epistemic drift and the driving forces behind it. In short epistemic drift may now be defined as the phenomenon whereby external pressures

and norms begin to take the place of internal cultural values and norms within scientific communities. If the process of epistemic drift - to which academic drift is a countervailing tendency - proceeds far enough the boundaries between the respective spheres of responsibility of science and political decision-making or commerce may be dissolved.

A third dynamic factor contributing to present day tendencies to epistemic drift has been analyzed by Regis Debray in his book Teachers, Writers and Celebrities. The Intellectuals of Modern France (French edn. Paris 1979, Engl. 1981). Debray makes a tricyclical analysis of the history of French intellectuals and their power in society. He sees a shift of focal power from the academics (dominating 1880 to 1930), to the publishing houses (1920-1960), to the media intellectuals which rose in the ascendence in 1968 and continue to rise to a position of at the top of a "pyramidal culture in which a mediatic elite became paramount over a subordinate publishing sector and an abject educational system" (ibid., p xiii of the Introduction written by Francis Mulhern). Debray borrows concepts from Bourdieu to characterize the mediatic revolution. According to him it has already had the effect of transmuting the traditional value system and quality control criteria within, at least, the humanities and arts domains of academia. "This sudden rise to power of an apparatus that was formerly subordinate or peripheral has had the side effect of shattering the coordinates of the 'intellectual field', taken here to mean 'the system of social relations within which creation takes place as an act of communication'. The order of its coordinates has changed, but inversely. Spatially there has been a broadening of the base; temporally, a narrowing" (ibid., pp. 80-81). And again: "The mass media have broken down the closure of the traditional intelligentsia, together with its evaluative norms and its scale of values". An example is the emergence of the so-called New Philosophers. If they had been obliged to organize themselves into a school and provide themselves with a review before making themselves known, Debray says, "they would have had to invent a philosophy, no less. Which would, ipso facto, have made them non-profitable in the eyes of their backers" (p. 83). And he goes on to compare with the Sophists of ancient Greece who outflanked the philosophers of their time, not only by developing a mastery of discourse as a power technique (rhetoric) but also by putting truth to a vote in the market place ("with the agora as judge"). In 1950, no philosopher would have dared to publish works like those of the "New Philosophers" which have been all over the glossy magazines. In 1950 they would have feared ridicule from within the academic community itself; the academic community was "still sufficiently compact to exercise self-censorship". Today however we have a situation where these internal control mechanisms embodying internalist epistemic criteria are being replaced by criteria of the media market.

This is exactly in line with what I have referred to as the phenomenon of epistemic drift; the only difference is in content and orientation, which of course will vary between disciplines and utilization contexts.

Hybrid communities

Van der Daele, Weingart and others (1977) have gone on to study how external goals and successive "translations" of these into researchable tasks become "internalized" into the research community. The planned and sectorially oriented mandated science of our time is sociologically characterized by the emergence of special groups of

people who function as brokers or intermediaries between the respective domains of responsibility of science and policy-making (politics). These carriers of the processes of mutual adaptation between the two spheres of interest and endeavour are called hybrid research communities (for more on the concept of "mandated science" in this context see Elzinga, 1985b). In Sweden they are populated by scientists, planners, bureaucrats with one foot in science and one outside it, representatives of private capital and commercial interests, trade union organizations, and other interest groups in Swedish corporativist society. The "hybrids" provide the translation process, the mediation, by which social and political goals become internal steering factors in research programmes, and one of their tasks is to keep research programs "on track".

In order to succeed in their translation tasks and implement the directives of their superiors, the "hybrids" have at the same time to represent the interests of the research community vis a vis the government Ministry (the state), the trade union, the industrial interests with a stake in the project, and/or some other interest groups at various levels (for example Municipal interests in high technology development in a city hospital). They have to see to it that adequate incentive structures are built up inside the research community, satisfying calls for positions, resources and recognition. In other words a good "hybrid research community" that is associated with a particular field of research is one that is also successful in representing the interests of the research community and getting money, positions and resources. This process when it takes more organized forms will constitute new self-reproducing career-tracks and recognitional systems. Richard Whitley has analyzed this as "reputational systems", comparing traditional disciplinary and newer interdisciplinary reputational systems. The traditional systems may be largely internally oriented (science without a clientele), while the newer systems based on mandated science are more externally oriented. Traditional systems are ones where reputation accrues through internal recognition and peer review; in the new reputational systems reputation may be linked to recognition of services to an external economic or political elite.

The new reputational systems that we see emerging today are to a large extent based on projects, externally defined programs, interdisciplinary networks and centers linked to sectoral interests or important issues. From the point of view of cross-national comparisons, Sweden is particularly interesting because of its strong corporativist networks. These tend to influence the formation of externally oriented research specialties. There exist well developed channels of communication and concensus-building which link science and societal interests. As Clark has correctly observed the state plays an important (dirigiste) role. In other countries where higher educational resources are more varied and scattered over larger geographical areas, academic oligarcies and/or market forces tend to have a stronger and possibly more immediate bearing on the carriers of the hybrid translation process, the broker function. In cases where there exists a federal structure and strong regional (state or provincial) determinants in higher education (for example Canada), one may find alliances of interests between the different levels (e.g. academia and federal government against regional stakeholder interests, or in Canada today with a Conservative government a stronger interplay of central government and private industry against academic and regional strongholds).

It would be interesting to compare in some detail - for example on the basis of some case studies of particular fields of science and educa-

tion in a number of countries - how hybrid research communities and networks function in different national and cultural settings. One point of comparison would be to see who populated the hybrid communities, because in their composition one may get some reflection of the various stakeholder interests. Differences in composition might be linked to relative differences of power amongst stakeholder groups in a country (state agencies, private capital and commercial market forces, municipal investments, and so on). In the Swedish case today one finds certain phenomena that bear resemblance to developments elsewhere: the emergence of regional industrial policies based on congruence of local commercial and municipal interests (Volvo and Gothenburg or Uddevalla city on the Swedish west coast); and the new winds of commercialization. One might speak of a regionalization and commercialization. Of course we have had a regionalization earlier. The emergence of fillial colleges in various parts of the country are products of regional political policies; but these have often been driven by politics and actors in the center. What is perhaps new is the greater independent strength of some regional actors and their ability to formulate and develop local technological policies more independently of the center.

As I have indicated earlier, I believe that this process constitutes a challenge to the scientific community, forcing it to become more policy conscious and formulate strategies which will assist to maintain the integrity of science and higher education, while at the same time opening up to new types of societal utilization. (A further discussion of what this may mean more concretely in connection with the establishment of a technology park at an engineering sciences faculty or university is found in Bärmark, 1986.)

The emergence and establishment of new disciplines

There is an extensive literature on the process of specialization whereby new disciplines emerge. Sociologists have developed theories regarding fashions in science; how and why at given conjunctures research workers tend to congregate around specific themes or migrate from one field to another. M. Mulkay has shown how new specialities sometimes emerge by virtue of paradigm invasions. A paradigm that has developed in one area may invade another, giving rise to a dynamic process of innovation. This is essentially a sociological view.

Focusing on the sociological dimension one can also sometimes find a kind of interfoliation of research efforts, networks being built up around certain topics or themes. This may happen spontaneously, or it may be the result of organized efforts. In the establishment of Linköping University's theme-oriented research organization we have an example of such a phenomenon on a large scale.

In the emergence of a new research and higher educational specialty it is not uncommon to find controversies and rivalry as to the contents and theoretical orientation of curricula and research efforts. An example is the liberation of our own subject, the theory of science, from its mother discipline, theoretical philosophy in Sweden. Even today there are philosophers, trained in the Anglo-saxon analytic tradition who, with their general knowledge of philosophy of science and specific competence in logical analysis, claim to have aboriginal rights as it were in the theory of science, regardless of the fact that the subject has developed into a direction of strong affiliations with the history of science and sociology of science, and regardless of the fact that they themselves to a large degree lack the large

measure of empirical grounding in on-going science in order to really "do" theory of science. Representatives of the mother discipline still want to influence the definition of the new subject and populate "positions" in the field. This may be interpreted as an attempt to establish administrative, social and epistemological hegemenony in the new research field. In a small country like Sweden, where Departments in the humanities are small and the total research population is limited, such tensions can cause considerable strains and growing pains for new subjects and their "social carriers".

An interesting example of the emergence of a new specialty in Sweden is that of nursing care research, mentioned before. In the establishment of this subject, where professorships as yet are few, paradigms in traditional fields like medicine, but also sociology and pedagogy are apt to invade and capture the intellectual field, while at the same time bureaucratic perspectives may colonize the conceptual apparatus. Thus it is important to discuss integrity and independence of such a new subject "nursing care" generally, or "health care research", or again "ward oriented research". Ultimately it is a question of who is doing the defining: nurses, physical therapists or bureaucrats and administrators. Each and everyone has a stake in the new subject. Nurses and therapists as groups see it as a vehicle for increasing their status and clout vis a vis doctors, and are in disagreement amongst themselves whether or not to favour a softer hermeneutic type of knowledge production or to let natural scientifically patterned modes of investigatory activities dominate. Thus the quibble about the terminology, what to call the new research field is not neutral. Rather it reflects strong vested interests on the parts of different groups of actors. The phenomenon may also be seen as a conflict over the content and direction of scientification of the health care professions.

In Sweden we have seen a host of new disciplines like nursing care research emerge during the last decade. Often it is the result of a mix of complex factors - professionalization, but also internal scientific specialization, and pressures from established power centres as well as from grass roots movements which push from below. Some writers maintain that peace and conflict research as a specialty in Sweden emerged largely due to pressure from the peace movement. The environmental movement is generally held to be influential in activating environmental research. But as movements' demands are taken up and translated into researchable problems, there is a tendency that the research may become encapsuled and loose its original radical sting. We see this too in womens research, originally stimulated by a cultural feminist movement, but now to a certain extent incorporated into the mainstream academic organization, the university. At the same time, as in the case of environmental research, there is a tendency also to become encapsuled in the corporativist decision making and policy making machinery, when womens studies for example are turned into a more gutless genre of research called equality oriented research ("jämställdhetsforskning") to suit the "definitions" of corporativist funding bodies.

Thus we find in the emergence of some new disciplines a confluence of various different, sometimes contradictory interests and stakes: popular grass roots movements, academic disciplines or rather their traditional knowledge "bearers" with their own traditions to defend; professionalization of specific groups of highly qualified or skilled persons in society; and a variety of corporativist interests. Many of the new disciplines that emerged in the seventies in Sweden, did so on the basis of external corporativist or professional stimulation, or

else thanks to pressures from below, from popular movements. In both instances it is thus more a question of external motive factors than internal ones.

In our study on the emergence of nursing care research in Sweden we suggest three main types of innovation:

- a new discipline develops by virtue of research being focused on a new area (territory) of reality;

- a new discipline emerges when an earlier field of research and its territory is seen in a new light (perspective shift);

- a new discipline emerges when new types of knowledge and skills need to be developed.

As noted before, it would be useful to do comparative cross-national studies to further investigate these types of phenomena.

National styles in foresight

In the history and theory of science the theme of national styles in science is beginning to receive some attention. In this genre there is a possibility of advocating both a stronger and weaker version of the thesis that national cultural, economic and political differences leave their mark on science and higher education. According to the weaker thesis such factors only affect the direction, rate of growth and organisational structure of research and higher educational forms, while the espistemic contents of science and higher education is unaffected. The concepts and theories as well as their truth claims are universal.

According to the stronger thesis cultural, economic and political factors as well as differences of organisational structure tend to affect the epistemic structure and contents of research and higher education. The various programs that seek to develop social epistemologies of a more radical kind would have to argue this.

At our own Department in Gothenburg we have for some time now investigated the milder version of social epistemological formation of knowledge structure. Andrew Jamison in his dissertation National Components of Scientific Knowledge (1982) makes a comparison of the historical evolution of natural sciences in Denmark and Sweden. In Denmark he finds a significant trend of romantic thought with roots in a "science for the people" movement associated with the educator Grundtvig, while in Sweden Linnéan biology has a more imperial style. The differences, Jamison argues, are related to the different roles of the two countries in a global context, and to differences in the class structure and mode of industrialization. Thus Jamison brings out differences of "style" in science which he seeks to tease out against the background of distinct metaphysical traditions, institutional structuring of scientific activity and the economic and industrial life in the two nations.

In another work, Cultural Components in the Scientific Attitude to Nature: Eastern and Western Modes? (1981), Jamison and I argue that at a very general level certain cultures can be more conducive to foster-

ing scientism while others are more resistant to this tendency. Although we find differences between philosophical thought in East and West, we cannot find any evidence for the thesis that equates positivism/scientism with Western science and learning, while tying mysticism/romanticism to Eastern traditions of learning. Indeed in both Chinese and Indian civilizations one finds a high level of scientific activity early on, but one also finds both more technocratic and more romantic tendencies there today. Both tendencies can gain nourishment from indigenous forms of philosophical and technological practice in earlier times. In this study we side with Joseph Needham in his critique of those historians and philosophers of science and civilization who see only different ethnosciences and deny the essentially eucumenical character of modern science. Like Needham we insist that the ethnoscience thesis lands one in a Spenglerian view of the natural sciences of the "various dead (or even worse, the living) non-European civilizations as totally separate, immiscible thought patterns, more like distinct works of art than anything else, a series of different views of the natural world irreconcilable and unconnected. Such a view might be used as the cloak of some historical racist doctrine, the sciences of pre-modern times and the non-European cultures being thought of as wholly conditioned ethnically, and rigidly confined to their own sphere, not a part of humanity's broad onward march" (Needham, 1954, p. xxxviii in the Foreword to vol. V). Needham's point indicates that it is not politically indifferent how one views the interrelatedness between socio-cultural and knowledge structures.

Let me now turn to the question whether or not one can find differences in national styles of "foresight". The term foresight is used to denote long-range forecasting of a kind that is distinctly different from the global modelling and trend extrapolation techniques of more than a decade ago when forecasting emerged out of think tanks around Pentagon to become an operational planning instrument in the civilian sector. Much of present day foresight methodology is based on "subjective" visionary activity and as such it allows the flavour and content of cultural values and patterns as well as different political structures to influence how these things are done.

In many advanced industrial nations today, and also in some developing countries, there is a wide range of activities that now go under the heading "Foresight in Science and Technology". One example is the programme for Forecasting and Assessment of Science and Technology (FAST) in the European Community headquarters in Brussels; another is ATAS, the Advanced Technology Alert System organized by Centre for Science and Technology for Development at the UN in New York. Within the National Academy of Sciences in Washington for some years now there have been Research Briefings, annual reports in which experts in select fields review trends and possible breakthroughs in their specialist areas of endeavour. At an earlier stage the National Science Foundation had its rounds of Five Year Outlooks, and Congress had its own version of foresighting, both at the Office of Technology Assessment and in the Congressional Research Services office. In each and every case the purpose is to make long range estimates of scientific and technological developments and their possible social impacts (impacts seen both as opportunities and threats).

In Sweden we have the National Board of Technological Development (STU) doing similar things in conjunction with the Engineering Sciences Academy (IVA), and the Natural Sciences Research Council also does foresighting in its area of competence and responsibility.

I am not going to review the various models and experiences with foresight here, except to say that in many cases the latent ideas is that one can somehow "pick" future "winners", or at least mould a consensus around investments in particular areas of science and technology that are perceived to be of social benefit and national advantage. My own experience in Canada in setting up a unit for Emerging Science and Technology and running workshops has convinced me of this (cf further Elzinga & McCutcheon, 1985 and Elzinga 1985b). A review of the foresight literature also suggests that there are interesting differences of national style in such diverse activities, and that these reflect differences of political structures, academic traditions as well as cultural and institutional arrangements in the different countries.

Ben Martin and John Irvine, in their book Foresight in Science. : Picking the Winners, describe the approach taken in France, West Germany, the USA and France. France has a dirigiste tradition, Japan less so, while in the US, long-term research forecasting is much more of an ad hoc activity within various government-related agencies. In each of these countries the aim is to identify promising areas of strategic research. In each case the end result of the exercises is more or less the same - there is a convergence on a number of major clusters of emerging science and technologies: microelectronics (and telematique), biotechnology, advanced industrial materials (metal-plastics, composites, conducting materials, etc), new energy technologies, research into surface physics, catalytic processes, and a few more. The methodologies in all four countries reveal considerable differences, but still there is this convergence.

In passing it may be asked how this very process of foresighting (and evaluation of research and higher educational systems) is affecting the structure and content of science? There are some indications that such activities contribute to a stronger political direction and produce accountability pressures that may affect these systems adversely. Also the convergence may to some extent be a case not only of objective growth patterns in international science and technology, but also the effect of subjective factors like taking the latest fashions in the US or Japan all to seriously. In Sweden in particular there has been a long history of tailing after North American models in the social sciences and in certain types of scientific methodologies during the post Second world war period and the days of Marshall help. Indeed it sometimes appears that some intellectual traditions in the Swedish academic world received their formative boost through a kind of intellectual Marshall help. This has however not helped make the Swedish research and higher educational system more pluralistic in organizational terms.

In France today, the Ministry of Industry and Research has a record of being dirigiste. In the late 1970's it initiated technology consultation exercises. In 1982 a national debate on the future of science and technology and their societal impact was started. This later resulted in a series of National Colloquys on Research and Technology. More recently the French Planning Commission together with the Research organ C.N.R.S. has set up seven independent groups of experts to think about the next twenty years, and some interesting preliminary results were presented in Paris last year. To a large degree the results confirmed foresight scenarios in Japan and the US.

Generally the French exercises are organized with the participation of representatives from industry, government, academe and the trade unions. In short the French seem to succeed in combining clear cut

governmental dirigism with broadly based national consultations across the whole country.

In West Germany the practice is very different. The federated political structure with a two-tiered level of responsibilities, and a division of labour in science between universities, Max-Planck institutes and a network of National Research Centres complicate the picture. Academic science is largely basic and unfocused, while the Max-Planck institutes and the national centres target fundamental research. In the main, scientists themselves have considerable autonomy and also considerable initiative when it comes to foresighting long-term research. A typical comment from a federal Ministry Official concerning his agency's efforts: "In my view, we undertake no long-term forecasting of strategic research. What we do have is a very spontaneous and unstructured process - a self-balancing process based on a political in-fighting and bargaining." (cited after Martin & Irvine, p. 52). Within the private sector in Germany Martin and Irvine found considerable evidence of extensive foresight activities in the companies they visited. Both in this and in the low-key government sponsorship of activities in forecasting West Germany resembles the United States.

Significant in the US is also the lack of industrial involvement in the surveys carried on by the NAC's Panel on Basic Research and National Needs. The NSF on the other hand has been known to engage Arthur D. Little Co. and Batelle Institute as consultants to focus on industrial perceptions of technological opportunities and relevant basic research.

However these studies have apparently had "virtually no impact within NSF. Those we interviewed in the NSF had mostly either completely forgotten about these studies or were unaware that they had ever taken place" (ibid., p. 82). There is some indication that this may be due to a general inability to involve academic and industrial research communities in centrally planned and organized foresighting activities. Indeed some actors at the center are jealous of their French and Japanese counterparts.

Irvine and Martin's account of the Japanese experience is the most detailed and instructive of the four country reports. In Japan the Ministry of International Trade and Industry (the famed MITI) has a long track-record of successfully bringing together industry, science and government. The national Science and Technology Agency also projects long term guideline forecasts over a period of thirty years into the future. The MITI scenarios are generally known as MITI-visions.

The attempts to import Japanese structures with the hopes of copying the Japanese visionary approach based on systematic consultation and interaction at many different levels of society have failed in the West. A lesson that we can learn is that the Japanese methodology is very much undergirded by and presupposes Japanese cultural and organizational structures with a long history behind them. Ever since the late 19th century, with exceptions for the period of nationalist military expansionism, the mainstream philosophy has been one of "Western technology, Japanese ethics and morality". And it appears until now that the graft is still being successfully sustained.

In a final chapter Martin and Irvine make a cross-national comparison based on their case studies. Their general conclusion is that up-to-date background information on research trends throughout the world's main industrial nations, and covering (as well as involving) academic,

government and industrial research sectors is very important. Approaches to foresight may differ, but it seems that the implementation of foresighting activities in the various countries tends to further affect the structures of knowledge systems and possibly their social organization. It would be interesting to study this in more detail.

Concluding remarks

The main thrust of this paper may be condensed in four points:

1. Knowledge structures have specific characteristics that may be defined in terms of internal specialization and epistemic organization. Resistance to change cannot be entirely reduced to social and psychological factors. Or to quote Arie Rip's conclusion in reviewing the findings of the case studies initiated by the Finalization theorists: the cognitive state of a discipline and the nature of its internal regulatives are independent variables determining the attitudes of scientists towards external orientation. This implies that resistance of scientists cannot always be reduced by institutional measures (e.g. money, opportunities, career structures), and that science policy has to take the cognitive aspects of its policy object into account (Rip, 1981 p. 307).

2. External processes of stimulation, professionalization, sectorialization, etc do not effect organizational and epistemic structural changes directly in higher educational systems, but rather by mediation, for example via hybrid research communities and broker communities. To quote Rip again: the appearance of hybrid communities is an indicator of an attempt at political direction of science, while its success depends on the institutionalization of external regulatives in a manner compatible with internal regulatives (ibid.).

3. In many instances the phenomenon of epistemic drift is equally if not more important than that of academic drift, and adequate policy measures should seek to promote organizational and incentive forms that will help counteract both.

4. The balance of equity and exellence is important, and relative autonomy especially in the case of newly emerging disciplines, where paradigm invasion and bureaucratic (perspectival) colonization can undermine integrity and the independent development of the new subject. Arguments for autonomy may be based on a) moral principles such as the need for critical research potential; b) pragmatic principles that stipulate how good research results are only forthcoming if quality control criteria are upheld; and c) epistemological claims concerning internal regulatives and their characteristic status of independent variables determining the attitudes of scientists towards external orientation, be it under pressure of sectorial policies, commercialization or medial penetration.

The third type of argument if it can be developed is a very strong argument for autonomy.

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Thorsten Nybom:

RESEARCH ORGANIZATION AND RESEARCH POLICY IN SWEDEN AFTER THE SECOND WORLD WAR - A DISCURSIVE ESSAY

Introduction

Already on the first page of his very interesting and farreaching book - The Higher Education System - professor Burton R. Clark is pointing at the selective and exclusive perspectives, different representatives of the social sciences have applied in their studies of the development and organization of higher education and universities.(1)

This "particularism", and lack of cooperation and continuity has, according to Clark, not only produced simplistic and one-dimensional "explanations" of higher education. The unwillingness or inability to complicate the existing reality is also partly responsible for the spread and cementing of fundamental misunderstandings about the societal role and the formative elements of higher learning and research. And, what is even more ominious, these "scientificly based" misunderstandings and simplifications have now and again come to serve as one of the basic motivations for comprehensive reforms of the entire system of higher learning in many countries.

These constitutive flaws in research, often combined with a lacking understanding of the cultural and normative dimensions in the different national systems of higher learning, have sometimes led to the introduction of deliberate "dysfunctions" into these systems. Thus, "reforms" which were introduced in the name of .pa efficiency and rationality have sometimes, actually become counter-productive and disintegrative.

There is, as I see it, no compelling or factual reason to make any significant objections or reproofs to professor Clark's general, critical evaluation. Instead, it could be formulated even sharper and more general.

Accordingly, my paper will not be anything like a traditional "review article" and I will certainly not accept the role of disciplinary "watchdog", correcting the "historical" details in Clark's analysis. Because, if you accept Clark's general conception and point of departure, this particular type of commentary would not only be unfair but also rather pointless.

It must also be stated, that my ambition has nor been to present an alternative concept of the development of higher learning and research. Instead, starting in Clark's thought-provoking analysis, I will make a modest attempt to present and perhaps also to qualify some basic elements and processes in research organization during the last 50 years. Out of practical and professional reasons my discussion will concentrate on the Swedish case.(2)

Bureaucratization as a Principle for Societal Decision-making

In the following, I would like to try to describe a system, which has been extremely open to external penetration, not least because of its gradual and comparatively massive bureaucratization. The functions and

dysfunctions of the Swedish system of higher learning must, in my opinion, be analyzed in terms of bureaucratization, defined not as an administrative technique, but rather as a "Gesellschaftsorganization"(3): A general principle for societal decision-making and power distribution, which eventually could penetrate - and basically change - almost any subsystem or sphere of modern society. Among these subsystems knowledge production in general and traditionally organized research in particular are, to my mind, especially vulnerable to bureaucratic penetration.

I maintain that bureaucratization has fundamental implications not only for the organization of science and research, but also for the entire concept and definition of research, as well as for the self-consciousness of the scholars themselves. These structural facts have often been neglected in a discussion, that has been more or less obsessed with the questions of formal organization and representation.

The research community in Sweden has stubbornly maintained, that the most important thing, if one wants to guarantee autonomy and an optimal growth in science and research, was to secure the right to be represented - if possible by majority - in the different resource-distributing agencies.(4) The "relevance" and fundamental "truth" of this strategy was never really questioned in Sweden. Instead, it soon turned into an axiom that was considered to be valid "for all seasons" and for every sector of society, including the production of knowledge.

My ambition is not to put "the blame" for what has eventually gone wrong in the Swedish development on any particular actor, but I do maintain, that the societal process I am refering to has made it necessary to discuss the entire concept of research and research organization in terms of bureaucratization. All the more so, as this appears to be a process, which has transcended the more narrow field of higher education and research. The expansion of bureaucratic power into politics has further loosened the relations between "relevant social knowledge" and traditional research, up to a point where pure ideology has become synonymous to basic "Sozialwissenschaft".(5)

Science and Research as a "Force of Production"

If one should try to comprise some basic features - or rather perhaps a "super-ideology" - of Swedish research policy after the Second World War, it could, with considerable simplification, be reduced to two postulates.(6) Firstly, science and research was a force of production quite comparable with - and sometimes even superior to - work and capital. This was something entirely different from the traditional concept, that scientific knowledge only mediately - i.e. in principle and in general - was responsible for the economic and social development of society. From the 1930s and onwards, science and research was directly and particularly linked to certain welfare, military and economic ambitions.

This further implicated, that science and research could not any longer maintain the claim of being treated as a special social and intellectual activity, with exclusive demands of freedom. From the second half of the 1940s such claims were considered not only as illfounded, but as an example of a lingering atavism: A more or less desperate attempt by certain groups - the chairholding professors - to preserve obsolete structures of power and organization.

The postulate that science and research should basically be seen as a force of production, included in many ways the second postulate of Swedish research policy: Higher education and research could and must be integrated in other social and economic activities. Society should intervene more actively and directly in research and research organization.

This did not just include the traditional tasks of overall national planning and financing of research. From then on it also included the practical organization and definition of science and research. Thus, the actively interventionist society put research and higher education at the top of the political agenda. Research was no longer just an evident object for political actions, it should also be considered as one of the sharpest and most efficient tools in the creation of the future welfare society.

This "ideological super structure" of Swedish research policy was by no means original or unique for Sweden. Quite the contrary, its basic elements can be traced in nearly all the industrialized countries.(7) Nor was this unreflected belief in integration, coordination and planning a Social Democratic prerogative. It was equally shared by all "progressive forces" in Swedish society after the war, not least by big business and industry.(8) The "discovery" of organization as the key to scientific progress and expansion had gradually also penetrated the scientific and scholarly community, beginning at the turn of the century.

The Swedish Universities and the Introduction of "Organized Science"

In the traditional Swedish university, the old collegial rule had started to crumble under rising numbers of students and the conscious and deliberate introduction of the German "Wissenschaftsuniversität". This development had begun roughly at the turn of the century. The new university statutes of 1908/16 can thus at the same time be seen as a codification of the primate of research and of academic autonomy, and as an illustration of the dubious consequences of intensified planning in science and higher learning.(9)

With the "Wissenschaftsuniversität" the collegiate system gradually turned into a formal hierarchy of representative bodies. The functions and competence of the all-embracing "Akademiska församlingen" were transfered via the "Stora Konsistoriet", where all professors no longer had access, to the "Lilla Konsistoriet" and "Drätselnämnden". This change, motivated by the rising demands of administrative efficiency and rational decision-making, could perhaps seem rather marginal and well within the limits of the existing system. But it nevertheless, meant an important and principal deviation from what had always been considered as the strong discriminatory assembly and rule of full professors. At the very moment one started to differentiate and delegate responsibility and influence in the name of administrative efficiency etc., there was an obvious risk, that the entire system of collegiate governing could be questioned.

As long as documented scholarly competence, or even eminence, was a necessary prerequisite to run the "business" of research, the university could at least hope, that its claims for autonomy would be respected. But when it came to administrative and management competence, the claim for exclusive rights would be very hard to uphold. Another ominious future implication of this development was, that the academic career could be split up into two distinctive and eventually even

antagonistic branches - a scientific and a polito-bureaucratic - with differing goals, ideologies and loyalties.

This was an internal academic process, which started long before the university got a more or less independent administrative apparatus of its own, run by professional bureaucrats. The initial and still rather rudimentary form of internal bureaucratization, by no means changed the relations of power or modes of organization in Swedish research and higher education. The university maintained its dominating role in the production of knowledge and in professional education and training, and the exclusive "rule of the chairholders" persisted more or less unrestricted.

There was another development within science, that soon enough proved to be even more important. I am of course referring to the emergence of the research institute and/or the departement, which really started after the turn of the century. This institutional concept was not to any significant degree an innovation brought in "from the outside", it must mainly be seen as a consequence of changes within science itself and as a result of the ongoing disintegration of traditional Academia.(10)

To start with, the introduction and creation of research based institutes was almost entirely limited to the natural sciences and medicine. Here one of the prime-movers was the accelerating disciplinary specialization. This, in its turn, was rooted in a scientific revolution, where the very concept of science, interest of knowledge and theory of science had become fundamentally revised. The rapidly rising costs and the need to use expensive instruments, large laboratories and professionally trained, technical and administrative personnel, constituted a constant incentive to develop the existing organization.

The pursuit of modern natural science became virtually impossible, if it was not organized as a thoroughly planned, collective effort with a sophisticated division of labour and responsibility. This did not just apply to the more spectacular cutting edge activities, but also to the normal science, that should eventually confirm the theories of the few revolutionary minds. It was an activity, which, from then on, usually included serial testing and large-scale experiments. The "new" science thus, demanded a different kind of competence and personnel, that could hardly be found within the traditional paternal "master-apprentice" organization.

The smashing success of the natural sciences was accompanied by a new, and apparently superior, form of organization. This eventually led to what one could characterize as a classical error in logic, or an "inverted" syllogism, which nevertheless was to constitute the fundamental "truth" in modern research planning and research organization in Sweden for years to come. It also became a basic element in the new "Wissenschaftsideologie", accepted by almost everybody within the research planning and research producing sector of Swedish society.

Thus, there was a pronounced tendency to explain the rapid and massive expansion of the sciences, both quantitatively and qualitatively, by its superior organization and well-developed specialization. The bureaucratic dreamland, where the production of eminent science and research was basically a matter of organization, had finally been born - at least as a hypothesis - and the Manhattan Project and Bletchley Park subsequently, turned it into a trivial and eternally relevant "truth". To what degree this development also would become destructive or at least distortive was mainly due to the disciplinary and "cultu-

ral" strength and self-consciousness of the organizations, that eventually were confronted with it.

A New Concept of Scientific Expansion and Implementation

There is yet another element in the research debate of the 1930s and 1940s, that in a decisive way affected the development of research policy planning. An element, which is necessary to consider if one is to understand the speed and significance of the ideological shift.

The debate during the Second World War did in many and fundamental ways change the entire concept of scientific development and expansion in Sweden. Until then, the concept of scientific expansion had been - what I would like to call - "horizontal": A continuing specialization and diversification into new fields of research. The new disciplines and subdisciplines had been considered as mainly an internal process, primarily motivated by the "genuine needs" of research, a "natural" consequence of the expansion in scientific knowledge.

In the period between the two world wars, there emerged nothing less than a new definition of research and scientific work, which expanded the research cycle to include (social) engineering and practical, commercial application. This extended definition was a concept, that tried to percieve the production of knowledge "vertically": The "scientific cycle" came gradually to include everything from basic research to the immediate practical, industrial, political and social application.

From now on, one really started to break the unity of science and to distinguish between different forms and levels of research, and in Sweden this developed into more than a mere taxonomy. The vertical differentiation was not only considered adequate for deciding the degree of "practical relevance" in a particular field of research. It soon also developed into the proper and universally adequate principle of solving problems in research organization and research management.(11)

In this development one could probably also find a genuine ambition to protect some vital parts of traditional research from external pressure. On the other hand, there seemed to be no anxiety whatsoever, that the vertical division could also eventually become a threat to the claims of autonomy, even if the vertical principle actually implicated, that science and research were, partly, prepared to trade its scientific ethos for a more or less utilitarian one.

In the vertical organization there was a definite risk, that the traditional claims of control and self-governing no longer seemed legitimate. Social actors or oranizations, which with increasing frequency and persistence refered to the immediate social usefulness of their work, and to their superior capability to produce ideological, economic and instrumental "commodities", could not - at the same time - uphold a legitimate claim to be treated distinctly different compared to nearly every other societal actor. In such a world "the occupation of curious gentlemen or of ingenious minds"(12) had to give way to a system, where strategic decisions concerning the content and the quality of research gradually became a prerogative of external, bureaucratic and political forces.

With the vertical organization of science and research, the homogenous scientific career came to an end. Instead there gradually appeared a

broad spectrum of "research bureaucrats", whose status and influence were not primarily connected to any form of disciplinary eminence, but rather to their bureaucratic and/or political position: that is, to what degree their production of knowledge covariated with the existing short-term welfare and economic goals. In these "hybrid" careers formal competence, institutional affiliation and professional loyalty could vary almost infinitely.

The "Deprofessionalization" of Knowledge Production. A Responsibility of the Swedish Research Community

The "process of professionalization", which this growing new cadre of "social technicians" went through, was only marginally identical to that of the traditional professions. The "new professionals" belonged, almost exclusively, to organizations or organizational settings, which were submitted to their traditional, professional collegues. They were employed primarily as bureaucrats and not as independent specialists with an exclusive disciplinary competence.(13) This was of central importance when private and public bureaucracies gradually developed from recievers into immediate sponsors and/or purchasers of research, and eventually, even turned into the most important and powerful producers of "socially relevant research".

The structural changes of influence and power in the production of knowledge had fundamental implications for the traditional academia and university organization. More and more the university was treated as a "basic resource": A service institution with the primary task of providing the different political and economic sectors of society with well-trained personnel, and to supply the demands of "instant research" from the powerful private and public bureaucracies - usually on a pure commission basis.(14)

The gradual "Verwissenschaftlichung" of administration and policy making should not automatically be seen as an irreversible and all-embracing process in modern western history. Its actual speed and impact varied between different sectors of society and between different fields of research. The process cannot be reduced to a mere question of resource allocation, as has been usually done in the Swedish debate on research policy-planning.

Resource allocation has been important, but I do maintain, that its consequences have been more or less indirect. In a research system, which initially was dominated by the representatives of university based research, there was almost immediately after the war, a deliberate introduction of bureaucratic principles in research organization. The number of intermediary bodies for "planning", "policy making", coordination, etc., multiplied. "Interest representation" soon replaced scholarly eminence as the leading principle in research councils et al. Thus, the distribution of grants and funds gradually became a process of negotiation between different interests, rather than of scientific gate-keeping.

The creation of new disciplines and subdisciplines was no longer exclusively or primarily a matter of research. It was to a high degree dictated by short-term "social" and "economic needs". If one wants to be a little bit unfair, the scientific expansion in Swedish social research could be described as a pathetic ambition on behalf of the research community to keep up with the agenda and priorities set by the ephemeral and everchanging "public debate".(15)

This development, together with the relative scarcity of resources, also brought about a gradual up-grading of short-term and otherwise limited research projects. The strategy could easily be defended within the research community as the only, or by far the most effective, way to secure funds and influence, and subsequently as the best way to minimize "unemployment" among active researchers.(16)

If one tries to break down this process into separate sectors of society and different fields of research, it is obvious, that the implications, manifestations and chronology differed. Thus, one could generally state, that the social sciences were relatively more afflicted than were other fields.

In its most extreme forms, this developed into something of a total symbiosis between research, bureaucracy and politics. This did not just mean, that research was almost exclusively commission based. It also meant that the different roles and loyalties of the researchers, the bureaucrats and the politicians became blurred. The most eclatant examples of the symbiosis in Sweden are Pedagogics and the Economic disciplines.(17)

Swedish Pedagogics and Educational research can be said to have emanated from the perpetual reform activity after the Second World War. The rapidly growing prestige, and the ill-founded - if explainable - self-esteem was directly linked to the alledged direct, practical usefulness of the disciplines and not to any scholarly or scientific expansion. Thus, in a changing political climate they could very well become victims of their own "socio-political" success.

There was no hard core of "scientific" knowledge or a solid disciplinary organization to prevent a process of decline and disintegration, when the "external" benevolence and backing stopped in the 1970s. If the negative implications of this process had only affected the "disciplines" concerned, it would have been of minor importance. But their decline and intellectual collapse, had consequences for the entire field of social research.

The structural, or rather institutional, dangers, which lay in the rising political and practical status of certain disciplines, were recognized by very few within the Swedish research community.(18) As far as Economics was concerned, there are also a couple of obvious explanations, as to why the dangers of "societal integration" were not taken seriously.

Firstly, the first generation of modern economic "advisors" were very few. Secondly, they all - almost without exception - posessed a solid scholarly competence, and an explicit sense of academic loyalty. These things were to change profoundly during the expansive 1960s, when competence was substituted for "relevance".

The demand-led mass training of different types of vocational "social technicians" created a substanstial cadre of "Halbgebildete", who gradually embodied the new "socially relevant researcher". Their integration in, or at least affiliation to, powerful private and public bureaucracies, did not only mean a growing influence, it also gradually changed their loyalties and their basic scientific ideology.

As stated above, one general dimension of this bureaucratization of research was the introduction and establishment of entirely new disciplines and fields of research. Many of these new disciplines, institutions and research fields, which were introduced in the Social Scien-

ces and Humanities during the last two decades in Sweden, have thus been motivated by welfare and other more or less instrumental societal needs.

Ultimately, this becomes a logical consequence if one regards science and research as primarily a force of production. But even more specifically, it also illustrates a clear disposition to concieve the expansion of knowledge as an immediate function of political and bureaucratic decisions and planning. In such a system the university and the traditional research community has no longer any right, power and special competence to occupy the central, strategic positions. A final step in this direction was perhaps the establishment of big integrated "research" institutions, which rather looked for "ideological/practical" competence than for disciplinary eminence in their recruitment and work.(20)

The Research Councils and the Triumph of "One-dimensional Thinking"

It is certainly no coincidence, that my discussion has centered upon the Social Sciences. As I have hinted earlier it is primarily these disciplines, that have been affected by the process of "bureaucratization". This could perhaps seem to be a rather bold and doubtful statement, inasmuch as it was the Medical Sciences and Technology, that had been the most eager and programmatic spokesmen for the "productive force" thesis.(21) But I maintain, that the ambitions and development in medicine and technology rather could be seen as a confirmation of existing facts, than as the beginning of an entirely new form of research and research organization.

This is partly reflected in the smoothness and speed by which these fields of research were reorganized after the War. In Medicine and Technology the organizational reforms and the increasing material resources could be unencumberedly transformed into scientific expansion. The role and influence of bureaucracy was limited to the administration of an otherwise independent and highly competent apparatus of "Big Science".

If it was more or less natural, that the Medical sciences and Technology acted as spear-heads in research planning and research organization after the Second World War, their example and indisputable success nevertheless created some fundamental problems in other parts of the research system.

As "nothing succeeds like success", and especially so in the theory of organization, management and planning, there was a strong belief, that the successful organization of medicine and technology would be directly applicable in other and totally different fields of research. Problems which did not exist in medicine, but which were crucial in other fields of research, were not even discussed: a) relations to society at large and to "practical relevance" in particular, b) the institutional setting, organization and principals of priority, c) the relation between "internal" and "external" resources, d) the necessary level and supply of competence. There seemed to exist a general conviction, that solutions, which had been adequate in one particular field, must be equally successful in another.

It is important to point out, that it was not the politicians or bureaucrats but the representatives of the research community, who most consistently emphasised these one-dimensional analogies. The only problem, that was identified and discussed among researchers, was the

eventual danger of political intervention in a research council type of organization; a problem, which actually did not exist at that particular time.(22) Anyway, before one discusses questions of "guilt" and responsibility, one must recall the circumstances; institutional, economic, personnel, etc., in which this new research organization was concieved and introduced.

The Swedish research council organization after the War was not only an important organizational innovation. It was also an administrative and a material intervention in an institutional system, that - to a considerable degree - must be seen as inefficient or even obsolete. The Swedish universities of the 1940s were not - by almost any standard - a well-functioning institution either for higher vocational training or for basic research.(23) The existing system was in such a deplorable shape, that almost any change would be an improvement, at least initially.

In an account of the Swedish research council organization one should make one principal, initial statement: The organization continued to be "value-rational", as long and as far as it was able to work according to its original principles and statutes. The main object of the research council system had been to create an extraordinary pool of resources in a system, where the basic needs of "normal" science already had been taken care of.(24) Accordingly, the councils had neither the money nor the duty to take any responsibility for the ordinary research work and the research training at the universities and colleges.

Until the late 1960s the proportions between the councils and universities were such, that the system could function according to the initial plans, at least in some research fields. The explosive growth in the number of "active researchers" - within and outside the university system together with declining (relatively) resources during the 1970s changed this situation in a fundamental way. The council organization was "forced" to make an impossible choice; they could either stick to their original responsibility of first and foremost promoting eminence in science and research, or they could use their limited means to support the basic university units in open crisis. The councils were primarily to choose the latter alternative, mainly out of misdirected - if understandable - loyalties to the university system as such.

The much desired "internal, scientific control" the research councils were said to secure, eventually developed into a clog of loyalties, which not only concealed, but even furthered systematic defects in various parts of the Swedish research system. The direct engagement in basic unit-maintenance and support, as well as the obvious and deliberate concessions to the powerful "sectoral" research organization, gradually transformed the research councils into bureaucratic authorities, which in a highly formalized way tried to "minimize" the misery of the basic units, by giving everybody - disciplines, institutions, subdisciplines, and individuals - their "fair share" of the limited resources.(25)

The Decline of the Swedish University

It might seem as if I have tried to project a picture of a process, which fundamentally transformed above all the social sciences and the humanities in Sweden after the 1960s. Such a conclusion would be entirely correct. Because even if the process outlined above in many

ways can be seen as universal, it was not identical in every western country. The impact and consequences for the production of knowledge in general, and for the traditional system of research and higher education in particular co-varied to a very high degree with the formative structures of the different national systems of research organization. Not least important was the position and status of traditional academia.

It is an indisputable fact, that the university has come to play a secondary role in the Swedish research system during the last 25 years than in most other national systems. The somewhat halfhearted attempts, made by different government comissions - for instance the "U55" and the "F63" - to establish proper research universities and to create something, which slightly resembled a professional, academic career, were never completed. Already by the time when the research council organization was first introduced, it was made abundantly clear, that the role of the universities in the coming research system should primarily be of a service or supply character. The universities et al should first and foremost supply the new organization with qualified manpower.

The arguments behind this standpoint were somewhat confused as regards the role and position of the university. On the one hand, it reflected a serious fear within the research community itself, that the freedom of academic research could be reduced if the universities were to be integrated into the new organizational framework. On the other hand, there was also a growing suspicion, that the traditional academia would be impossible and irrational in this planned, socially oriented setting.(26) This latent distrust of the university, by and by, became an explicit and important "fact" in the populistic and utility-oriented late 1960s.

The marked distrust as well as the misdirected care for the traditional university also meant, that Sweden abstained from using two of the most common and succesful means of organizing research after the Second World War: the university-based Research Institute and the independent but university-integrated Graduate School.(27) This almost programmatic refusal would eventually have fatal implications for the university as well as for traditional research in Sweden.

This general conclusion could also be illustrated by some crude and rather cold figures: In the early 1970s the basic grants for research in universities and colleges had declined to something between 15 and 25% of the public R&D-budget. Of the gradually more important "external" money, the share of the basic research councils went down from 50 to 35% during the 1970s.(28) Thus, at the end of the 1970s the Building and Housing Research council (BFR) administered more money, than did the University of Stockholm and The Royal Institute of Technology.(29) The hope, expressed by for instance the 1972 "Research Council Commission" (FRU), that it was necessary, "to solve the problems of the close relations to traditional university-based research" in Swedish research policy planning, had been more than "adequately" fulfilled.

Formation and Structural Characteristics of Swedish Research Policy after the Second World War

Instead of a "proper" conclusion, I would like to try to present the process outlined above in a more systematized way. Rather than being a mere resumé, this should make it possible to discuss the Swedish

research policy of the last decades in terms of formative elements and basic structures. At the same time, I would like to be a little more specific as regards the consequences for the traditional university system.

The dynamic decade immediately after the War has traditionally been described as a massive build-up and reconstruction of basic research and the university organization in Sweden. These statements are indeed far from incorrect, but one must also discuss if that was the most significant and formative element in the emerging new research apparatus. To my mind, this was not the case. By far more important for the future, than the new resources to basic research were the fundamental principals and ideological premise behind the massive and sometimes impressive reform activity.

One of these "fundamental principles" seem to have been, that rational planning and production in research demanded a new and elaborate hierarchy of decision making and a streamlining of different functions. Between the existing, granting (parliament/government) and research producing (universities) levels, a distributive and policy making level was gradually introduced, that was basically bureaucratic - both in character and in function.

In spite of its research-dominated composition, the research council did not represent any gathering of strength from below. The councils should mainly bee seen as supervisors appointed or commissioned by the state.(30) They should guarantee, that the growing resources were used in the most efficient way possible, and that the general goals of the new research policy were realized.

This request on part of the central political authorities, to have "ultimate control" was in no way improper, and it had always been accepted by almost everybody within the system. The crucial thing is, that nobody apparently understood, that the new system had different possibilities and (potential) implications compared to the old one.(31)

The formula introduced by the councils served as a model for most of the future research planning and research financing authorities and institutions, but the influence of traditional science and research became gradually weaker as the bureaucratic element got stronger. Thus, the organizational innovation (research council), which was described as the "ultimate solution" of the eternal conflict between society's wish for control and the research community's claim for maximum freedom, gradually but surely turned into a legitimation of a rather comprehensive bureaucratization of research, that really started to accelerate during the late 1960s.

The extremely rapid growth in the number of research administrating and research planning bodies, which characterized Swedish research policy even before "sectoral" research became the official doctrine, actually meant, that the original research councils within a decade changed from strategic centres of gravity to junior partners in the national research system.(32)

Thus, in the middle of the 1970s the research councils were treated almost as "anomalies"; problematic, university-affiliated bodies, which were difficult to integrate into the dominating system of sectoral research. So, even if the 50 or so new bodies of sectoral research, that emerged up to 1980 can justly be said to have had the research council as its organizational model, it is just as obvious,

that their affiliations, loyalties and competence, in reality were not only incompatible with, but even antagonistic to, the original research ideology behind the councils.

The antagonism became all the more obvious, when these sectoral "research" institutions became fully integrated parts of their respective "mother" bureaucracies. In a very short time they developed from sponsors of research, to planners of research and ultimately to fully fledged "research" bureaucracies with their own hybride ideology and polito-bureaucratic, normative system.(33)

This was bound to have heavy repercussions on the behaviour of the original research councils. From the outset the main responsibility of the councils had been towards basic research. They should primarily be the guardians of scientific quality. Secondly, they should act as scientific advisers and experts in research planning, but they should not take on the role of procurators or entrepreneurs of research, (soon to become a standard operating procedure in sectoral "research").(34)

Politicians as well as active researchers in Sweden either came to ignore, or at least they greatly underestimated the structural and ideological consequences of commissioned research. The opinion, that sectoral money becomes "harmless" as soon as it is channeled through the university or its affiliated institutions, is to my mind such a mistake. The "strengthening" of the basic unit, that external, commission money was supposed to bring about, was in most cases a best nominal; it increased the number of researchers employed and the number of pages produced but virtually nothing more.(35)

Structurally, and qualitatively, the effects were often the exact opposite. It furthered and accelerated the disintegration of academic organization. It reduced administrative and intellectual flexibility. It promoted ad hoc-planning and encouraged tendencies of more or less systematic opportunism. Last but not least, it also speeded up professional disintegration.

All this together, created a type of problems, that had to be solved mainly by bureaucratic means, which further reduced the flexibility and research capabilities of the institutions. A self-supporting and all-consuming machine had finally penetrated traditional research in Sweden.(36)

However, the most fatal implications of the Swedish research policy process are, that the entire concept and status of science and research have been affected. The rise and eventual victory of sectoral "research" has meant, that traditional research to a very high degree has taken over - or at least accepted - the political and bureaucratic definitions of what is to be considered as "socially relevant research".(37)

The original definition of "social relevance" had been formulated by society as a whole, and had been directed towards the collective research community. The ongoing "sectorization" - or better "bureaucratization" has meant a gradual perversion of the entire concept of "social relevance", until it has become identical with the acute needs of short-term planning and of day-to-day politics.

Subsequently, the actual "expertise" in research planning had been transfered from the representatives of disciplinary competence to bureaucratic specialists. The planning and organization of research

changed accordingly. It was integrated into a system, that wanted a maximum of "practical research" in the shortest time possible. This had also an immediate and deep-going effect on the principals of the distribution of funds and other types of resource allocation in traditional academic research; a) A highly formalized system of choosing between relevant and "worthy" research projects was introduced. b) Political and bureaucratic needs and values became decisive in creating new fields of research. c) Bureaucratic control and accounting of research grants became standard procedure.(38)

Incidentally, this redistribution of power did not only affect the status and conditions of traditional research, it was also one of the crucial preconditions for a qualitative leap in the process of bureaucratization, which is a distinct and formative element of modern society, namely Structural or Transsectorial Bureaucratization; the sectoral principle did not just mean, that bureaucracy penetrated research. It also meant, that a certain type of "research" became a necessary feature of a new type of bureaucracy.(39)

The development of sectoral "research" and bureaucratic research planning can also be said to have created its own "bureaucratic dynamic". The sectoral concept included two fundamental thoughts, which were contradictory and thus in themselves a constant promoter of bureaucratization. The sectoral research strategy claimed to promote both specialization, stream-lining and practical relevance in descision-making, and to guarantee maximum integration in sectoral, political and administrative planning.

But the first strategy, which was supposed to further "decentralization" and "tailor-made" results, tended to lead to such a degree of disintegration, that the much wanted "overview", "control", "co-ordination", etc. became virtually impossible; it ran counter precisely to those qualities, which sectoral organization was supposed to bring about. The second ambition of total, sectoral integration, on the other hand, due to lacking external scrutiny and to an obvious professional incompetence, led to intellectual stagnation and irrelevance.

One would perhaps think, that these obvious failures, should have been a sufficient indication for those responsible, that the new "master plan" had some serious, built-in dysfunction. But "luckily" enough for the bureaucracy in charge, the difficulties could also be interpreted as the result of too little bureaucracy, thus instigating yet another round of bureaucratization in the research system instead of re-thinking.(40)

This self-generated need for either "coordination" or "decentralization" became obvious at a very early stage in the process. Already before the research council organization was fully established a "Delegation of Cooperation" was erected.(41) But gradually coordination changed from primarily an administrative technique to an independent, bureaucratic level with great variations in form and functions, including everything from ad hoc committees, policy making institutions (e.g. Forskningsberedningen, 1962) to established state authorities (e.g. FRN, 1977).(42)

The Swedish Universities in the New Research System - A tentative Evaluation

Finally, I would once again like to return to the role of the traditional university in the production of knowledge during the last 40

years in Sweden. I do this mainly because it is interesting in itself, but also because it could further illustrate the ideological and organizational shifts, that have taken place. One could present the case of the Swedish university either by an analysis of the recommendations and proposals made by the numerous government committees from 1945 to 1977, or by discussing a number of hard figures. I will mainly stich to the first variety.(43)

Initially, I would like to repeat what I consider to be a fundamental and an undisputable fact in Swedish research policy after the Second World War; the secondary role played by the universities and colleges. From the late 1950s this tendency can be said to have turned into a basic principle of research organization. The important and formative changes took places outside and not seldom also against the traditional system of knowledge production. Academic research surely got a share of money and other resources, but one must remember, that the main bulk of these resources went to research conducted outside the traditional disciplines. Or, at best, it went to activities only indirectly linked to, or controlled by university research.(44)

As a general description, this could by some be considered as a little bit unfair, in view of the substantial sums of public money and the amount of political energy, that were spent on the Swedish educational system - and not least on the university organization - after the War. I maintain however, that this particular interest only marginally included the vision of creating a traditional research university. The discussions and propositions were mainly about the organization and management of basic training in the coming Welfare society.

The Swedish development must of course partly be seen as a "natural" consequence of hard "physical" and structural facts, which by their sheer weight were able to produce fundamental changes in the entire system of higher education: The increasing number of students, the division of labour and the rapidly transforming labour-market, the private and public, bureucratic expansion, etc.(45) But their transformation of the university system, must also be seen as ideological actions. A fact that is perhaps even more obvious in the Swedish case with its tradition and ability of active public intervention.

In the very ambitious and professor-dominated "University Commission of 1945" (U45) the traditional research university, and the Mertonian concept of science and research as a specific activity was very much predominant. Organization as well as curriculum should be subordinate to the imperative demands and norms of research.(46)

For the members of U45 the main ambition seems to have been to reorganize and reactivate the traditional university system. But in spite of its six extensive reports, and the sometimes qualified analysis, it is clear that the practical result and the ideological importance were rather limited. It was obvious that the future belonged to the rising research council type of organization. Thus, the possibilities of the university to play a leading role in the coming expansion of research was removed almost from the very start.

The next comprehensive university reform - U55 - was to have much greater consequences. U55 meant nothing less than a fundamental change of the entire system of higher learning in Sweden. From the top - the new coordinating authority of "UKÄ" - to the bottom - the formal introduction of partly independent "institutioner" - organizational and functional innovations were introduced.

The main task of U55 had been to solve the very acute and real problems, which the rising number of students in basic training had created. In this work U55 must, by almost any standard, be considered as very successful. It designed a university organization, that for at least 10 years functioned much better than all of its predecessors and followers in Swedish university history.(47)

What made U55 so successful was probably the fact, that it identified the "right" problems and used the right means to solve these problems. And what is perhaps even more important, their ambitions were realistic. The reforms proposed by the U55 had three basic dimensions: a) A substantial reinforcement in money and personnel in basic education. b) The introduction of a professional bureaucratic apparatus in the academic system. c) A "rational" division between undergraduate teaching and research.

Adequate and successful as these changes might have seemed at the time, the reforms of U55 also entailed elements, which in many ways opened the gate for a gradual disintegration and bureaucratization of higher education. One such element was the bureaucratic - not rational - division between basic education and research activities, including research training. Another was the introduction of the so called "automatique" principle, which meant that the number of teachers was to expand in relation to the number of students. This "temporary" measure meant an actual and deliberate reduction of competence in the university staff and a loosening of the organic ties between higher education and active research. Eventually, this could - and would - also, in certain disciplines, lead to intellectual "swamps" of more or less blatant incompetence. A third element was the introduction and legitimation of an independent administration with bureaucratic rather than academic loyalties.

In sum, I would - perhaps somewhat too categorically - state, that U55 not least through its obvious success created an almost superstitious belief in the almost "magic" power and eternal relevance of administrative and material growth. For a long while there was, in Sweden, a marked tendency to discuss almost every aspect of higher education in terms of administration and money.

This disposition was not only to be found among bureaucrats and politicians, but just as frequently among people inside academia. The thought that internal crisis and dysfunctions in research and higher education could be of cognitive or, intellectual or social origin, was hardly ever considered. The consequences hereof would start to appear in the late 1960s.

The "1963 Research Committee" (F63) can serve as an illustrative example.(48) In retrospect the committee could be seen as an almost desperate, last attempt to establish a functioning research organization inside the traditional university. By creating a larger and more flexible organization, with a combination of apprentice- and carreer positions, together with a formalized research training program, F63 was hoping to achieve an institutional base - large and dynamic enough - to constitute the necessary "critical mass", which is indispensible in any successful research organization.

These reforms were never to be realized, very much due to the resistance within the academic community itself.(49) An immediate consequence of the internal obstructions was, that a "UKÄ-Committee" took over and hammered out a "reform", which became a bureaucratic reform with virtually no substantial content. But it had one fundamental

merit in the eyes of the polito-bureaucracy - it was cheap. The UKA-committee made an arbitrary selection among the original F63-proposals: Four years formal research training, formalized and compulsory tutoring, a more or less symbolic number of new Research Assistant posts. And all this was done without any simultaneous reinforcement in money, personnel and competence, which F63 had considered to be an absolute necessity.

U68 - "Final Curtain or Strange Interlude"?

With its obvious aspiration towards formalization, its ambitions to build up and strengthen local and central bureaucracy, its stressing of general welfare goals, it is perhaps tempting to describe U68/H75 as a logical and final conclusion in a general development during the last three decades in Sweden. Yet, such a description would only be superficially correct, because in many and fundamental respects U68 was something entirely new in Swedish higher education, both with regard to ideological consistency and consequence, and to its composition and working order.(50)

This statement is not only due to the fact, that vocational training superseded research affiliation as the central dimension in Swedish higher learning ("line system", etc). U68 also introduced an entirely new set of fundamental goals for higher education; Ideological schooling (Democratic training, Critical thinking), Social welfare/Equality ("25/5-rule", Open access, etc), Regional justice (local colleges), etc.

As the main obligations of the university towards society no longer were scientific training and the production of scientific research, there seemed to be no reason why the organization, position, etc. of higher learning should be subordinate to the peculiar demands of research. Or why other societal actors beside government and the research community should not have every right to interfere in the affairs of the university.(51)

From then on this right belonged to anyone, who cared to use it, and soon enough almost every organization, corporation, political party, newspaper and "individual" journalist used this "civil right". This was, as I see it, a final break with the traditional concept of how the relations between the university and society should ideally be: Towards society there was an imperative "cultural" obligation, but towards the nebulous and opportunistic phenomenon called "public opinion" there should be no obligations whatsoever on the part of the university.

All this considered, and also with regard to the official, ideological declarations of the U68-protagonists, one could perhaps detect a new tendency in the production and valuation of knowledge in Sweden: The social engineering of the 1950s and early 60s was giving way to a more or less pure "Ideology-production" as the most relevant form of knowledge in modern society.

University research - already a minor entry in the total R/D-budget - was transformed by the U68-reform and its followers to just another activity in higher education with no legitimate special claims. To fulfill its duties or even to function modern higher education primarily needed bureaucratic and political know-how.

Subsequently, those today, who - with rising surprise and even anger - are watching the expansion of functions in the university system other than traditional education and research, have simply made themselves blind to the obvious: In the existing bureaucratic conglomeration called R&D, university research has not only lost in quantitative terms. But it has no longer any special "density" or undisputed normative function in what is commonly called "research". And what is more, it is not even master in what used to be its "own little house".

Another general consequence of the disintegration of Swedish academia is the disruption of the internal basic values and traditional loyalties in higher learning. I am here not refering to the "natural" alienation from the powerful local bureaucracies, neither am I discussing the unnecessary and artificial conflict between basic education and research.(52) What I am thinking of are certain tendencies within the community of active researchers itself.

The traditional loyalty towards disciplines and research as a specific activity, cannot indefinitely impede the process of social and intellectual dissolution, which growing differences in employment, social security, salary, influence, etc. constantly foment. In an organization where 20 individuals with roughly the same formal and real competence, may have at least 15 different forms of employment, such traditionally integrative forces as "alma mater", department and even discipline are bound to lose their power. In such a situation there remains, at best, a personal loyalty between individuals. This together with the obvious devaluation of academic research in general, could eventually, lead to a deminishing belief in what Thorstein Veblen once called "the professional Instinct". If and when an active researcher looses that, he has lost everything.

To "measure" the alleged detoriating position of university research in Sweden is of course a rather intricate task. There is however, one crude but fairly interesting "measuring-tape", namely salaries. Thus, at the beginning of the 1950s the salary of Swedish university professors was among the highest in the OECD-countries. In the early 1980s it was probably the lowest - with the possible exception of Portugal.(53) A comparison with other civil servants in Sweden would show a similar tendency. This could perhaps be dismissed as the usual lamentation from one with obvious vested interests. Be that as it may - the facts still remain.

NOTES

1) Burton R. Clark, The Higher Education System. Academic Organization in Cross-National Perspective, UCP 1983, pp. 1-10

2) (Many of the problems discussed in this paper will be reappearing in my forthcoming analysis of the Swedish Research Policy after 1945. This, together with its character of discursive essay, mean that the notes are to be seen rather as references than as traditional "evidence")

3) See Th. Nybom, "Samhällsorganisatorisk forskning: Max Weber och funderingar om en historisk byråkratiseringsteori", in Sociologisk Forskning 1985:4

4) See Th. Nybom, Forskningsråden växer fram 1942-1950, Uppsala 1985. Also Rune Premfors, Svensk forskningspolitik, Lund 1986

5) For a discussion see e.g. T. Nybom, Forskning och byråkratisering i Sverige efter 1945, (Rapp. till UHÄ-symp) Uppsala 1984. Also Th. Nybom, "Politikens kris i det Skandinaviska välfärdssamhället", in Nytt Norsk Tidsskrift 1984:4. And Aant Elzinga, "Research, Bureaucracy and the Drift of Epistemic Criteria", in Björn Wittrock - Aant Elzinga (eds.), The University Research System: The Public Policies of The Home of Scientists, Stockholm 1985.

6) The following is mainly a summary of Th. Nybom, Universitet och forskningen i Sverige, Uppsala 1984. The standard work in the field of research ideology is still Sverker Gustavsson, Debatten om forskningen och samhället, Uppsala 1971, (here esp. pp. 181-182).

7) See e.g. A. Elzinga, "Forskningspolitiken i Sverige", in A. Elzinga - Ronny Ambjörnsson - Gunnar Andersson(eds), Forskningspolitik i Sverige, Sovjet och USA, Stockholm 1969

8) Th. Nybom, Universitet och forskningen

9) Torgny Segerstedt, "Universitetet i Uppsala", in Uppsala stads historia IV, Uppsala 1983

10) See e.g. Günther Wendel, Die Kaiser Wilhelm Gesellschaft, Berlin 1975

11) In particular Th. Nybom, Universitetet och forskningen, in general S. Gustavsson, Debatten om forskningen och samhället

12) John D. Bernal, The Social Function of Science, p. XIII

13) For an intro. see e.g. Magali Sarfatti-Larson, The Rise of the Professionalism, Berkeley 1977/79. Also Amitai Etzioni, A Comparative Analysis of Complex Organizations, New York 1975, and Rolf Torstendahl, Bureaucratization as a Historical Phenomenon, Uppsala 1984

14) Th. Nybom, Forskningsråden växer fram

15) Th. Nybom, Forskningsråden växer fram, Peter Stevrin, Den samhällsstyrda forskningen, Stockholm and commission reports, Forskningspolitik SOU 1977:52, Forskningsråd SOU 1975:26. Also R. Premfors, Svensk forskningspolitik

16) For a discussion see Kjell A. Eliasen-Mogens N. Pedersen, Svensk samhällsorganisation och förvaltning: En inventering av svensk forskning, Oslo 1984

17) Ibid. On the internal discussion among Swedish social scientists see, Samhällsforskning. Rapporter från fem arbetsgrupper tillsatta av Forskningsberedningen, Lund 1968, and 20 års samhällsforskning, Statens Råd för samhällsforskning 1948-1968, Stockholm 1969

18) These "dissenters" could in fact be reduced to Gunnar Myrdal. On Myrdal and research policy see, Th. Nybom, Forskningsråden växer fram

19) For a complete and detailed list see, SOU 1975:26; also P. Stevrin, Den samhällsstyrda forskningen

20) Ibid. also above note 5

21) Th. Nybom, Forskningsråden växer fram

22) Ibid.

23) For a vivid description see the different enquetes and interviews made by the research council commissions (riksarkivet)

24) Th. Nybom, Forskningsråden växer fram

25) Most notorious is perhaps the so called "Thalberg-Formula" in SFR, see Th. Nybom, Svensk forskningsorganisation efter det andra världskriget, 1987(fc)

26) Th. Nybom, Forskningsråden växer fram

27) Ibid.

28) Katrin Fridjonsdottir, Vetenskap och politik, Lund 1983. Also SOU 1975:26 and SOU 1977:52

29) Walter Korpi, Särskilt yttrande till Byggforskningsutredningen 83-01-13

30) See R. Premfors, Svensk forskningspolitik and P. Stevrin, Den samhällsstyrda forskningen

31) Th. Nybom, Forskningsråden växer fram

32) See SOU 1975:26, SOU 1977:52. Also R. Premfors, Svensk forskningspolitik

33) P. Stevrin, Den samhällsstyrda forskningen

34) See Instruction - Kgl br. 59-06-30 (with changes and amendments) in SOU 1975:26 bil 2

35) For a discussion see, e.g. K. Fridjonsdottir, Vetenskap och politik pp. 107-145

36) SOU 1977:52 (esp chapter: Universitetens och högskolornas planeringssituation). Also SOU 1981:29, Forskningens framtid. Forskning och forskarutbildning i högskolan

37) See R. Premfors, Svensk forskningspolitik

38) See Th. Nybom, Svensk forskningsorganisation efter det andra världskriget (fc)

39) See Th. Nybom, Samhällsorganisatorisk forskning

40) See e.g. R. Premfors, Svensk forskningspolitik

41) (Forskningsrådens samarbetsorganisation was established in 1947)

42) An interesting and extremely expensive variety are the bureaucratic conglomerations labelled "Program". See Björn Wittrock, De

stora programmens tid - Forskning och energi i svensk politik, Stockholm 1983

43) For statistical figures see f.i. Jan-Erik Lane - Bert Fredriksson, Higher Education and Public Administration, Göteborg 1983

44) Jan Annerstedt, Makten över forskningen, Lund 1972

45) E.g. Rolf Ohlson, Högre utbildning och demografiska förändringar, Lund 1986

46) J-E. Lane - B. Fredriksson, Higher Education and Public Administration

47) SOU 1957:24, SOU 1959:45, SOU 1963:10, 11, 12

48) SOU 1966:67, 68 and Birgitta Odén, Forskarutbildningens resultat 1890-1975, 1987 (fc)

49) Minutes from internal discussions in SFR, and in different commis-sions (HSFR and RA)

50) Bo Lindensjö, Högskolereformen. En studie i offentlig reformpolitik, Stockholm 1981

51) See e.g. Mål för högre utbildning. U68-Debatt, Stockholm 1968. Also B. Lindensjö, Högskolereformen

52) On the relations between bureaucrats and teachers/researchers at the Swedish universities see, J-E. Lane, "Om byråkrati och byråkratisering", in Tvärsnitt 1981:1 and 3 and Lane, Rapp. to UHÄ-symp. Uppsala dec. 1984. Also NFR-rapp to SOU 1980:3

53) Bo Lehnert, "Forskarnas ekonomiska villkor", in Gunnar Ström (ed), Erövra universiteten åter, Stockholm 1984, pp. 57-70

Tony Becher:

DISCIPLINARY DISCOURSE(1)

Introduction

> "The limits of my language mean the limits of my world"
> (Wittgenstein 1922: Prop. 5.6)

This, like most of the sayings of Wittgenstein and other great philosophers, is open to a number of interpretations. I propose here to paraphrase it in a simple and direct way. If something is beyond the limits of my language, if there is no way in which I can communicate it, then there is a sense in which it cannot be part of my world. To put it in another form, the whole of what I understand in a meaningful sense - my world - is coterminous with my linguistic repertoire. The connections between understanding, communication, and awareness of reality are close enough - on this reading of Wittgenstein's proposition - to have the inexorable force of logic.

But if language so closely matches the known and knowable world, one might reasonably expect differences in fields of knowledge to be reflected in differences of linguistic form. It is to test out this expectation that the present paper has been written.(2)

There are several possible levels of approach. The most obvious, perhaps, would be to identify those terms which stand for the central concepts in each discipline, and to show that they do not occupy the same key communicative role within other disciplines. But this feature is too well known to deserve much elaboration. Physicists deal in fields of force, in quasars or quarks (depending on their macro- or micro-orientation); sociologists in role models and social stratification; historians in periods and primary sources. None of them share the same intellectual vocabulary.

Beyond such considerations, however, lie the acceptable verbal formulae which demonstrate the possession of an important form of tacit knowledge within a given discipline (Gerholm, 1984). Linked to this are differences in the basic values that different disciplines espouse, as shown in their common terms of praise and blame (Becher 1981). The characteristic modes of formal communication, as evidenced in scholarly journal articles, provide another significant indicator. So too - in a perhaps more fundamental way - do the conceptual structures of scholarly argument, serving to mark off the intellectual style of one discipline from that of another (Bazerman, 1981).

It may be useful to explore these different levels, or aspects, in turn. In order to keep the discussion within reasonable bounds, I have decided to focus on three contrasting disciplines - history, sociology and physics.(3) Where relevant, I will draw attention to the linguistic context and to the structure of discourse as well as to the language in use. The main sources of data for the argument are threefold: selections from the relevant scholarly literature; material gathered over the past five or six years as part of my investigations into the cultures of academic disciplines; and a small-scale analysis of the leading journals in each of my three selected fields. In relation to the last of these, it should be mentioned that I scanned four

journals each in sociology and physics, and three in history. I chose, arbitrarily, the volume or volumes relating to 1984 for an overall perspective, and the issues relating to January to April 1984 for more detailed consideration.

Tacit knowledge

Reviewing the literature of the sociology of science, Bazerman (1983) remarks that "Communication between participants in separate disciplinary matrices is rife with misunderstanding and unresolvable conflict - unresolvable because there is no neutral terminology that will allow for determination of mutually acceptable criteria of adjudication." In the same passage, he also observes that "because the shared features of a disciplinary matrix often lie below conscious articulation, writing within each discipline can only be fully understood by those who share the matrix." This comes close to Gerholm's (1984) account of tacit knowledge: "To function smoothly within the group of teachers, fellow students and secretaries, the student needs a considerable amount of know-how. Most of it will be acquired slowly through the interaction with others and without anyone ever making a deliberate effort to teach the newcomer the rules of the game."

By no means all tacit knowledge takes verbal form, indeed perhaps only a small subsection does so. This is one source of difficulty in attempting to incoporate it in a general study of disciplinary discourse. Another - even more serious from my point of view - is that, like Snyder's (1971) Hidden Curriculum, its recognition and elucidation necessarily depends on the patient and long-term techniques of participant observation: it cannot be identified by the type of in-depth interviews which my own researches have comprised (Becher, 1986).

This part of the discussion will accordingly draw heavily on Gerholm (1984). At one point he quotes an anecdote by the Swedish author Lars Gustafsson, who as a student of philosophy noted the riposte "That I don't understand!" to result in confusion and consternation for opponents in argument. Later, when Gustafsson left philosophy for a study of comparative literature, the same strategy failed to have any impact: "my admission of incomprehension was tactfully disregarded and the discussion continued as if nothing had happened." This seems to me a very good example, bringing out both the aspect of learned but not taught behaviour in tacit knowledge and its specificity of context.

To identify comparable examples in the fields of history, sociology and physics is not easy, given the limitations noted above. As it happens, Gerholm offers an example in sociology, quoting Kleinman (1983). The pressure on graduate students in sociology was for them to become "highly individual and original researchers... At wine and cheese parties, where they were being introduced to the department, faculty and older students would immediately put questions like 'What is your specialty?', 'What is your field of interest?' The effect was to reinforce the notion that, as one student remarked, 'it is pretty much assumed that at this level you work by yourself'."

Another possible instance of verbalised tacit knowledge in sociology is encapsulated in the question: "How large was your sample?" This is analogous with the Gustafsson statement in philosophy, in that it calls into question a variety of taken-for-granted issues: it implies a shaky empirical foundation for the substantive findings of the study; it hints at potential inadequacies in terms of methodology: and

worst of all it appears to cast doubt on the theoretical framework within which the enquiry was undertaken.

Both examples suggest a level of self-consciousness about professional procedures in sociology which are arguably less evident in the cases of history and physics. Again, Gerholm cites "Merton's established norms and Mitroff's underground rules for action" as spelling out key areas of tacit knowledge in the physical sciences. But these are all to do with ways of conducting scientific enquiry, rather than with linguistic expressions of disciplinary values. In contrast with the sociologists, one might see the physicists as needing no reassurance about the theoretical foundations of their discipline or the validity of its procedures, and as concentrating their tacit knowledge on matters of collective attitude and behaviour.

In history, I would argue, tacit knowledge commonly takes a different form again - that of folklore and gossip rather than of implicit rules of conduct or explicit concerns with epistemological status. Common professional interests focus on common archival sources: at this level, what the well-informed historian needs to know is where the best sources are for a particular topic; where to stay near the Bodleian; where to eat near the Bibliotheque Nationale. As one of my informants remarked, historians "talk more about the tools of the trade than the trade itself" (Becher, 1980). The virtual absence of "shop talk" is attributed to the fact that the nature of the subject lies in details: most problems cannot be adequately defined in conversation.

What this brief and superficial foray into tacit knowledge in history, sociology and physics does indicate is that historians show little explicit concern with method and theory because the discipline is largely atheoretical, and is seen in methodological terms as a craft to be mastered as much as a set of techniques to be learned; that physicists are for the most part equally unconcerned with fundamental questions of epistemology, because they take their discipline to be firmly based and not (except by those exploring fundamental questions about the nature of matter) open to fruitful debate; and that sociologists, in contrast, tend to be taken up with questions about the status of knowledge, the applicability of theory, the validity of method and the efficacy of technique. These distinctions will be developed and extended as the discussion moves into other areas of discourse.

Praising and blaming

The qualities that are considered desirable, and those that are to be avoided, vary from discipline to discipline. The terms commonly used for the purposes of approbation and disapprobation help to pinpoint disciplinary values and to mark the defining characteristics of any given field. It may be useful to consider such terms at two levels - first, that of ordinary conversation (Becher, 1981), and second, that within the more public and formal setting of the published review.

Among historians, "scholarly" and "original" mark qualities which are valued but difficult in their achievement. Other forms of approval are somewhat muscular in their overtones: "rigorous", stimulating", "masterly" and the like. Terms of condemnation designate weaknesses it is easy to fall into. The worst that can be said of a piece of work is that it is "trivialising" or "thin" - in that the evidence is inadequately deployed and fails to relate closely enough to the conclusion.

To designate an argument as "well researched" or "sound" is to damn with faint praise, since both are essential requirements of any worthwhile piece of history. Stylistic judgements are quite common: it is unacceptable to produce material that is "jargon-ridden" or "sloppy in style", while something which is "well-written" is to be commended. "Biassed" is not a term which is normally used, because "it is taken for granted that every historian is biassed in one way or another."

Sociologists share some of the same terminology. Thus, to designate a piece of work as "rigorous" or as "stimulating" is to accord it high praise; while to use the word "biassed" is merely to "betray one's naiveté - to say that a particular interpretation is slanted is not to say enough to be useful". "Scholarly", however, has slightly ambiguous connotations among sociologists, convening perhaps a hint of disengagement or even of scholasticism. But other evaluative terms differ from those used by historians: "well-argued", "powerful", "persuasive", "perceptive" and "thought-provoking" on the positive side suggest a particular concern with the quality of the analysis itself or with the impact on the reader, as against its substantive content. On the negative side, "anecdotal" and "contentious" suggest a corresponding emphasis on a lack of argumentative grip.

Within the community of academic physicists, "elegant" is a highly-prized property of a solution to a problem; and to say of a theory that it is "economical" is to imply that it is devoid of waste and packed with value. "Productive" denotes a piece of investigation which opens up a new set of issues or suggests a novel approach to existing problems. "Sloppy" is the commonest term of condemnation, indicationg a piece of work which lacks not only the desirable qualities of neatness and structural simplicity but also the necessary qualities of thoroughness and reliability. "Accurate" is a double-edged tribute, since no physics worthy of the name should lack the property. "Rigorous" is another backhanded compliment, implying a narrow, unimaginative employment of mathematics with no proper grounding in the physical world. Perhaps because of the fast-moving quality of many areas of physics, the terms "masterly" and "scholarly" seem altogether inappropriate, and are in fact never used.

These contrasting vocabularies serve to indicate some of the cultural differences between the three disciplines. One useful mental experiment is to substitute some of the terms from one disciplinary cluster in another. Critics of sociology might offer an obvious explanation why "jargon-ridden" is not a common term on condemnation in that discipline, even though it is in history. It is not quite so evident why "productive" is a form of praise in physics but not in history, or why "persuasive" is used in sociology but not in history or physics. But at the intuitive level, there is a sense of appropriate matching - as one might expect - between the epistemological properties of the subject and the language used to evaluate the research generated within it.

In turning from conversational comment to the public arena of the book review, a number of the same terms reappear. But the considered prose of the reviewers (or, as we shall see in the case of physics, its absence) reflects certain significant features of the academic culture in question, more through style and tone than through the vocabulary used.

The English Historical Review is a long-established and prestigious journal, most issues of which carry a very sizeable section - amounting to over half the text - of reviews and short notices. For the

most part, the historians' tradition of "civility and gentility" (Becker, 1980) is evident enough. Praize is generally unstinted and occasionally fulsome. One learns of "an appropriate tribute to a fine scholar", and ponders over a comment that "the book is so good that one would wish it even better." But occasionally, in a restrained and qualified way, a critical note is sounded:

> A book which, although repetitive and somewhat turgid in style, will provide a stimulating platform for future analysis.
>
> This book, as the first on an important aspect of history, must be welcomed and used. But it is a pity that it is a slight work with serious deficiencies.

American historians, as one of them observed, "don't have such suffocating good taste, though we do believe in being civil to each other." The acid is somewhat sharper in the American Historical Review (which again has more than half its pages devoted to critical notices). Among a large majority of comments of the form "this book should be taken seriously by practitioners of our craft" and "the writing is pungent and readable", one comes across such comments as:

> The work contains many other inadequate interpretations. The disjointed organisation of the book makes it difficult to detail them. Furthermore, there is excessive repetition ... This is not to say that the book is without entertaining qualities.

Or even, at the extreme of blunt statement:

> His approach to sources is so tendentious that his book cannot be recommended as a scholarly treatment of the subject.

Generally, though, there is much positive reference in the review sections of both journals to "sound scholarship", books that fill "welcome gaps" and those constituting "a valuable addition to the source material", alongside the occasional rebuke about "theoretical pretensions".

Reviews in the leading sociological journals are more robust and blunt in tone, and sometimes indeed frankly offensive. As in history, the majority of the comment is positive and polite. But my sample of the reviews in Sociology included the following observations:

> I wish the book had been less hackneyed, less ostentatious and more acute.
>
> Alexander's book is without originality /.../ (it) represents a failure even to understand the problems;

while those in Sociological Review yielded even more caustic and personal judgements:

> Tomlinson's chapter /.../ is confused in argument, superficially institutional in its choice of evidence, and idiosyncratic in content. The collection is a useful demonstration of the obvious.

> Cardechi /.../ strikes one as the man who has devoted his life to reinventing the wheel. Only his is square.

The American Journal of Sociology is almost mild in comparison:

> A narrow yet unthorough work /.../ the style is dry and unenlivened /.../ and many of its assumptions and assertions unproved and questionable.
>
> Given the cyclical nature of intellectual fashion, this currently unfashionable work will probably look better in a few years' time.

There is no adverse reference here - as among the historians - to theoretical pretensions, but only to "methodological weaknesses". One work is cited as "an essential guide to our growing understanding", suggesting a cumulative process which most historians and many sociologists would deny. On the whole, both disciplines are presented by their practitioners as critical and reiterative, in contrast with the more evidently linear structure of the natural sciences (Becher, 1986).

It is, arguably, the rapid accretion of knowledge in physics, with each new finding building on already-established results, which accounts for the absence of any noticeable tradition of book publishing (other than student texts and review monographs, which are regarded as outside the main stream). And in the absence of books, there are no book reviews either. The nearest approximation to a critical literature is the thematic survey of a particular topic, such as can be found in Reviews of Modern Physics. Here, each contribution offers a conspectus and synthesis of the main recent contributions ot its chosen theme, with a list of references which will normally run to two or three double-column pages. Almost invariably, the cited papers are discussed in factual rather than evaluative terms, the only obvious form of criticism being omission. The purpose of the exercise is described by the editors in the following terms: "we /.../ believe that a healthy review literature is vital to the physics community, providing one of the chief modes of communication among physicists of widely differing fields."

As with specific terms of praise and blame, certain disciplinary differences emerge in even this brief analysis of review writing. In physics, as has already been implied, the recently-published material in a given field is regarded more as a contribution to an accumulating corpus of knowledge than as a series of intellectual products in their own right. This collectivist tradition is less appropriate in fields such as history and sociology, where the problems are not closely defined and the structure of knowledge is not clear-cut; where argument takes the place of demonstration and judgement substitutes for proof. Individual appraisal of the work of another individual is part of the business of maintaining and monitoring the "common assumptions and styles of thought" enjoyed by historians (Becher, 1980): poor quality work has to be identified, while work of acceptable quality is enthusiastically praised and promoted. In sociology, the critical and evaluative function would seem within the disciplinary community to be linked with what sociologists have variously described as "the convention of internal dissent", "sophisticated techniques for demolition", "polemical arguments", "factionalism", and even at times "personal dislikes and rancorous accusations". This in turn seems likely to derive from the sociologists' strong dependence on theoretical struc-

tures, combined with the wide variety of competing theories (and the transient nature of many).

Formal scholarly communication

Articles in learned journals are a near-universal form of communicating academic findings and transmitting scholarly ideas. Yet their superficial similarities barely conceal important differences of presentation and structure from one disciplinary field to another. In this section of my argument, I propose to examine and compare the contextual features of journal articles in history, sociology and physics before looking at some of their functional components, as manifested in the language they employ. I shall defer until a subsequent section a look at what, paraphrasing Chomsky, one might call the "deep structure" of journal articles - that is, their forms of construction and the purposes of their arguments.

Even the most cursory glance at, say, the Journal of Physics, the Physical Review and Physical Review Letters is enough to mark them off in several respects from the English Historical Review and the American Historical Review, as well as from Sociology and the American Sociological Review. In the first place, the authorship of papers in physics is much more commonly collective than individual. The pattern seems to vary somewhat from specialism to specialism, but to take extreme examples, one article in Phys.Rev.Letters 52, 14, 2 April 1984 had eighteen authors (from three different US locations); in the same issue, there were only two authors writing individually out of 22 contributions. Co-authors are also geographically scattered: thus one contribution came jointly from researchers in Brussels and Warsaw; another (in Phys.Rev.A 30, July-Dec 1984) from a tripartite group in Manhattan (Kansas), Aarhus and Calcutta. Historians and sociologists are noticeably less collaborative and less cosmopolitan. In Engl.Hist.Rev. 99, 390, Jan 1984, there is one contribution by two joint authors out of a total of 21 - and that is a Short Note rather than a full article. In the American counterpart, Am.Hist.Rev. 89, 1, Feb 1984, there are 21 solo authors and only 2 joint - one of the latter pairs writing on a somewhat technical subject in the history of medicine. The sociologists' proclivities for joint authorship are somewhat greater: in Sociol. 18, 1, Feb 1984 there are 15 single as against 6 multiple authors; in Am.Soc.Rev. 49, 1, Feb 1984 there are as many as 15 multiple as against 13 single authors. In no cases, however, do the multiple authors number more than three, or come from locations in more than one country. The basis for this particular contrast would appear to lie in the fact that, in history, "ideas have to be shaped in the mind of the individual scholar from a first-hand examination of documents, rather than from a second-hand study of other peoples' reports", while insofar as sociology also involves interpretation, one may want to ask: "how can three people write with one pen and speak with one mind?"

The length of papers also varies between the science and non-science journals. In Sociology the norm would appear to be around 20 pages - somewhat longer perhaps in Am.Soc.Rev.. In Engl.Hist.Rev. and Amer. Hist.Rev. the length is rather greater, with a norm of about 30 pages. In Phys.Rev.Letters, at the other extreme, no communication is allowed to exceed 3 or 4 pages. In Phys.Rev. the lengths are very variable between specialisms, ranging somewhere between 5 and 18 pages. In Jnl. of Phys.C., covering solid state topics, contributions may sometimes touch the 20-page mark. The physics journals are fragmented in a way that their historical and sociological counterparts are not. Thus,

Phys.Rev. has four separately published sections (General Physics; Condensed Matter; Nuclear Physics; Particles and Fields), while Jnl. of Phys. has seven (Mathematical and General; Atomic and Molecular; Solid State; Applied; Scientific Instruments; Metal; Nuclear). This subdivision into different fields is perhaps inevitable, given the large volume of publication. To take a dramatic example, Phys.Rev.B. (somewhat ironically devoted to Condensed Matter) averages a publication of between 500 and 600 pages every fortnight throughout the year. The measured quarterly pace and relatively modest volume of the leading journals in history and sociology perhaps offer a useful reminder that physics is in comparison a large-scale academic industry with a rapid and highly international communication system, embodying a very different disciplinary community from theirs.

Turning from context to content, one difference is so obvious, and so familiar, as to seem hardly worth mentioning - though again, it seems to underline a fundamental difference in epistemology. To the uninitiated reader, only a few isolated passages in any physics journal article are in plain text, the residue being in code. That is to say, mathematical sympolism plays a very substantial part in the discussions and demonstrations of physicists, in a way in which it markedly does not in historical or sociological writing. American sociologists would appear (from Am.Soc.Rev. at least) to be more quantitatively-minded than their British counterparts, scattering their texts with the occasional regression table, list of coefficients or catalogue of means and standard deviations. They also tend to espouse such quasi-scientific terms as "parameters", "exogenous variables" and (most widely-used of all) "models". Historians (at least those outside economic history) use virtually no quantification. Pictorial illustrations are employed sparingly - in some branches of physics, photographs of particular phenomena, scatter diagrams and graphs illustrate the argument; historians provide the occasional map or reproduction of a contemporary print; sociologists appear hardly to use visual communication in their articles.

Where the written language of physics is impenetrable to the outsider, that of history is easily accessible - at least on the surface. That is to say, historians couch their discussions almost entirely in familiar, everyday language. Sociology tends to be less readily intelligible to the layman, since it commonly involves the use of such technical or semi-technical terms as cultural resources, deviant, false consciousness, frames, ideological, paradigm, role, structure, socialisation, social reality, and the like. Some sociologists interviewed in the course of my research, while acknowledging the "mystification and protection" that jargon affords, excused it as "a useful short cut"; and one argued that because theoretically-oriented sociologists try to work at a level which frees them from the constraints of time and space, they need to develop some form of universal language, striving to encapsulate a complex phenomenon in a single word. One might go on to argue that, because historians value particularity as against generality, they have no comparable need for a specialist language of their own.

Writers in all three fields make fairly generous use of references, but - as might be expected - use them in systematically different ways. As heirs to the Germanic tradition, American scholars, in sociology at least, produce lengthy bibliographies - their British counterparts less so. Physicists, except when writing synoptic review articles, are also somewhat more modest. Historians use a great many references, but the majority - as I shall shortly go on to argue - serve a purpose particular to their discipline. In the leading history jour-

nals at least, comments and citations appear as notes at the foot of the relevant page, rather than as separate sections of notes and references at the end of the text. The latter seems to be the practice in physics and sociology, though Am.Soc.Rev. has a half-way system of footnotes and end references.

The historians offer a variety of reasons for citation (Becher, 1980): "it is professionally important to acknowledge relevant work by other people"; it "serves a social and institutional as much as an epistemological function"; it enables you to engage in debate with other writers, protects you from reviewers' criticisms, and more generally operates as a mechanism for preserving professional solidarity. After all, "if we don't read them, perhaps they won't read us". More fundamentally, however, references to primary and secondary sources "give the reader a sense of the interplay between data and argument - an almost literal sense of tension as the reader's eye moves constantly between the one and the other" (hence the strong preference for notes on the same page - the tension would be seriously slackened by a constant turning of pages to and fro). In sociology, the baser motives for citation are claimed to include quoting famous people to show that one keeps good intellectual company, and giving a vulgar display of erudition. But one might also want (Platt, private communication) to contest a rival position; provide a brief review of the relevant literature; define a topic by indicating potentially relevant areas not covered; or draw attention to an interesting but neglected source. One is in any case morally bound to acknowledge a direct quotation from another published text. Physicists would appear to have two main forms of reference. The first, in common with historians and sociologists, is to demonstrate one's familiarity with the relevant literature and provide a context for one's own work within it; the second is to show that a technique or procedure used in the process of argument is already established as effective. Once more, the contrasts here serve to underline the differences between the disciplines: the expectation in history of close patterning on the primary data; the more volatile nature of sociological argument, and the complementary problem that there is no sense of superseded knowledge in sociology, and the tight mesh of research in physics, moving almost inexorably from one set of findings to another.

A frequently-noted feature of scientific prose is its predominantly passive voice - though a recent study (Tarone et al, 1981) suggests that this view needs to be modified, in that we, plus an active verb, occurs in several contexts. The first person singular tends to appear in what might be called asides or stage directions, rather than in key passages in the argument. A similar reluctance to use "I" rather than "we" appears in formal passages of sociological analysis, as opposed to more narrative sections. The first person singular is understandably obtrusive in contexts in which the author wishes to give the argument an impersonal - and by implication objective - flavour. One can indeed distinguish, in physics and in sociology, at least three distinct senses in which the word "we" is used: the grandiose (meaning I, the author, in a more than singular guise); the collective (meaning we, the joint authors); and the cooptional (meaning you, the reader, and me/us, the author/s). Historians are fortunate in being able in large part to avoid such devices. Their articles tend to put forward an exposition in which the author plays a mediating role between the historical narrative and the source material, becoming visible only in occasional footnotes expressing a personal view, or in parenthetical comments such as "I attempted in an earlier paper to..." It may not be altogether fanciful to discern again in these differences the self-effacing historical craftsman dealing in particularities, the more

public and competitive physicist dealing in universals, and the sociologist questing for laws of behaviour while recognising that the world of human affairs is one of infinite variety.

Structures of argument

In looking at the way in which arguments are constructed in various disciplines, one has - so to speak - to look through the language at the cognitive structures which lie behind. A fairly direct and straight-forward approach to the analysis of academic writing was adopted by Hartley and Knapper (n.d.), developing a strategy attributed to Lowenthal and Wason (1977). They sent out a questionnaire to colleagues in their respective institutions - one in the UK and one in Canada - asking for autobiographical comments on procedures adopted in academic writing and attitudes towards the writing process. The results, based on a modest-sized sample of 63 responses, did not bring to light any clear differences between academics with different disciplinary allegiances, save in one respect. Those on the arts and science sides "appear to differ in how they go about writing as a consequence of why they write and how they structure their approach. Artists appear to go more for the argument, working from the 'top down' as it were, whereas scientists seem more inclined to write individual components and work from the 'bottom up'." The authors speculate that "this may be connected with the greater need in artists /.../ for intrinsic rewards (the satisfaction of sorting out one's ideas and developing one's thinking) compared with the greater need in scientists for extrinsic rewards (getting into print, advancing one's field etc.)."

The finding is an interesting one, but gives rise - in the light of the suggestions put forward in earlier sections of this paper - to an alternative, or perhaps an additional, line of explanation. Pure science, as exemplified in physics, is based on a process of accretion of knowledge, and hence may be said to be atomistic (each new piece of the jigsaw has to be identified and fitted in to what is already there). History and sociology, being in their different ways based on criticism, reiteration and reinterpretation, do not match the jigsaw metaphor. Knowledge here is organic and holistic, growing in a complex and comparatively unpredictable way (Becher, 1986). So a scientific writer, having the rules of the game already established, knows the components and structure of his argument before he embarks on committing it to paper, and can thus afford to asemble it (from the bottom up), piece by piece. In contrast, in the more interpretive disciplines, the explanations may well have to be constructed in relation to the particular topic, taken as a whole: it is therefore far from easy to piece together the argument from separate and distinct components, as against conceiving it as a totality "from the top down".

A more elaborate analysis of academic writing has been developed by Swales (1981, 1983), concentrating on the introductory sections of scholarly articles in a number of scientific and social scientific journals. Swales was able to identify a fairly consistent four-stage pattern, in which the author first sets out to establish his or her field; next summarises previous research; third, prepares the way for the present research; and finally, introduces the present research. He fleshes out the bare bones of this structure as follows:

> We know from everyday experience that if somebody tells a story or relates an anecdote in a conversational group, at the closure of the story, one standard way of continuing the

> interaction is for some other member of the group to tell another story or relate another anecdote. Of course, this second story cannot be any story; in highly complicated ways the second narration has to share certain informational and thematic features of the first story, and generally speaking, and up to a certain point, the more it has in common with the preceding story the more the conversational group will accept it as a natural and appropriate new long turn in the conversation. Further, the second story will be expected to extend some aspect of the first story - in the classic instance it will attempt to 'cap the previous anecdote'. (Swales, 1983)

Applying this analogy to academic publication, Swales contends that the researcher's introduction

> will be, amongst other things, his version of the relevant previous research to be written as a 'first story' so that his original 'second story' can extend or 'cap' it. Now in order to achieve a successful second story (in order to obtain a round of academic applause), our researcher has to proceed with certain care. For one thing, he needs to establish that the composite first story is 'cappable' by his own story; therefore he needs to ensure that his second story is preceded by remarks and hints that demonstrate that the composite first story of previous research is not a complete or completely successful one. Equally importantly, he needs to show something else. He needs to establish early on that all the stories are serious and non-trivial - that the combined story is relevant to wider issues whatever superficial indications there may be to the contrary. It also follows, of course that the weaker the second story, the more careful must be the first story preparation for it. (Swales, 1981)

This account fits neatly with the articles in Phys.Rev.Letters 52, 14, 2 April 1984. In particular, the third stage (of "story-capping") is illustrated there by a number of similar strategies:

> Here we show that this restriction can be removed.
>
> Not much experimental work has been done on this topic.
>
> It would seem that the unitary picture /.../ is simply too naive.
>
> A serious limitation of (this) approach is /.../ In this letter, I present the first practical method of determining...

The Swales analysis also fits reasonably satisfactorily with a number of the articles in Am.Soc.Rev 49, 1, February 1984 - a journal which, as has already been remarked, favours a quantitative and quasi-scientific approach. However, the leading historical journals tend not to require formal introductory sections - the authors usually plunge directly into their chosen themes; and where introductions are provided for articles in Sociology, they follow no consistent pattern. The notion of "story capping", illuminating though it maybe, clearly depends on a field in which knowledge either is, or is presented as being, cumulative. It underlines the need for academic authors in such fields to establish the credentials of their work in terms of what has

gone before - a necessity which historians are spared by the very nature of their task, and which sociologists can avoid unless they wish to adopt a scientific guise.

Swales (1983) raises a further issue which is directly relevant to the theme of this paper: namely that writing an introduction is emphatically not "an activity totally governed by objectivity and reason and devoted to straight reportage of previous research and judicious appraisal of the place and merit of the writer's own research." Indeed, "many article introductions are essentially exercises in public relations /.../ beneath (the) surface they are pleas for acceptance and designed accordingly." For all that has been said about the clear rules of procedure for, and the contextual limitations of, argument in fields such as physics, there remains scope in the presentation of findings for the skills of entrepreneurship and the techniques of impression management.(4)

Looking at scholarly writing from the standpoint of social psychology, as Hartley and Knapper have done, or within a linguistic framework, as has Swales, can thus yield useful insights into the nature of academic knowledge and the communities who generate and purvey it. However, the most valuable contribution I have so far come across to an understanding of this theme results from applying the methods of textual criticism to articles in three different academic fields. Charles Bazerman, in his study of "What Written Knowledge Does" (Bazerman, 1981), insists that we should neither view scholarly writing "singly as a theoretical problem in communication", nor abstract our analysis "from the writer's task of embodying complex meaning in a specific text." He proposes instead to approach his three chosen texts from four perspectives: "the object under study, the literature of the field, the anticipated audience, and the author's own self."

The article is rich in insights: but for my present purpose I will concentrate particularly on those comments which underline key epistemological features or which draw significant contrasts between the three different fields. The fields selected by Bazerman happen to fit reasonably well with the disciplinary themes of this paper: molecular biology (which has certain similarities of structure with physics); the sociology of science (a recognisable sub-section of sociology as a whole); and literary criticism (which, like history, is a "mainstream" arts subject, though with some important differences from it). The texts he chooses by way of exemplification are J.D. Watson and F.H.C. Crick's famous paper "A structure for Deoxyribose Nucleic Acid"; Robert K. Merton's study of "The Ambivalence of Scientists"; and Geoffrey H. Hartman's discussion of "Blessing the Torrent: On Wordsworth's Later Style".

Beginning with an examination of Watson and Crick, Bazerman observes:

> the primary context explicitly attended to by the language of the paper is the context of the objects of nature /.../ the authors' apparent presence is minimised by the common pursuit of authors, literature and audience to establish a common, codified, symbolic analogue for nature.

But the sense of depersonification goes further: "The object is taken as given, independent of perception and knowing: all the human action is only in the process of coming to know the object - that is, in constructing, criticizing and manipulating claims." Even the process of verification is impersonal: "The other researchers must satisfy

themselves that the model fits past evidence and new tests. It is up to nature to persuade the readers, not the authors."

By way of contrast, in Merton's article:

> a wide range of linguistic choice is open to the author: little is predetermined by a knowledge of reality codified in language, literature and the criteria of judgement. Merton must develop at length original formulations to represent the phenomenon, to assemble and interpret the relevant literature, to establish his perspective, and to attend to the audience's perception.

Earlier, Bazerman has remarked that "The author's perspective is /.../ in many respects individual: nonetheless, through the medium of the paper he hopes to establish his claims as shared knowledge."

Verification is here a more complex procedure: "The unsympathetic reader /.../ can find in unconstrained meanings enough inconsistency, contradiction and unacceptable thought to mount a serious attack." In this case, indeed, "language, rather than being highly determined by the discipline's shared perception of reality as it is in the Watson-Crick article, must be carefully shaped by the author to turn his own vision into the shared one of the discipline."

Finally, in Hartman's study,

> The poem, the literature, and the audience's perception are all mediated through the critic's vision. The critic perceives new dimensions of the poem, uses the literature to allude to his own aesthetic experience, and asks the audience to accept a new way of reading the poem. The poetic text and its context, the accumulated experience of literary criticism and literary texts, and the audience's critical judgement and expectation of poetry do constrain what the critic can persuasively state, yet the critic has considerable power to transform all of them.

Here, verification takes on a special meaning: "The final test of Hartman's argument is whether it illuminates the poems. No hard evidence will determine whether his is right or wrong /.../ all the argument is based on plausibility /.../ and even notions of plausibility can be changed if the essay succeeds in expanding the reader's poetic imagination."

In reviewing and generalising his findings, Bazerman writes: "The chemical and biological literatures are codified and embedded in the language, problematics, and accepted modes of argumentation /.../ The sociological literature on scientific behaviour is more diverse, unsettled, and open to interpretation: therefore, the essay must reconstruct the literature to establish a framework for discussion /.../ The literatures of poetry and its criticism tend to be particularistic and used in particularistic ways /.../ Codification, if it can be called that, is entirely personal."

These observations, it seems to me, helpfully extend and elaborate the comments in earlier sections of this paper. Although history cannot neatly be substituted for literature in Bazerman's analysis of the Hartman article, it is nonetheless evident from even a brief and selective scanning of the English Historical Reviews, Past and Present, and the American Historical Review that the historian's vision

is central to the interpretive process. Indeed, one might paraphrase Bazerman in commenting that "The historian perceives new dimensions of the primary source material, uses his secondary sources to illuminate his own historical interpretation, and asks his audience to accept a new way of understanding the past. The nature of the source material, both primary and secondary, and the audience's professional judgement and conception of history do constrain what the writer can legitimately say, yet the writer has substantial scope to modify the prevailing view."

Conclusion

Although many other things could doubtless be said, the considerations advanced in the preceding pages will I hope have gone some way towards establishing the usefulness of linguistic analysis - taken in its broadest sense to include such treatments as Bazerman's - in illuminating our understanding of the nature of academic disciplines, and hence of what Martin Trow has called "the private life of higher education".

No attempt will be made here to enumerate in detail the cultural and epistemological distinctions which have been pointed up between physics as a pure science, sociology as a social science and history as a humanistic study. It seems to have emerged quite clearly, however, that the three disciplines display fundamental differences not only between type of evidence and procedures for proof, but also in the characteristic ways in which the work of previous scholars is deployed, in the terms in which evaluation of others' work is communicated, and in the modes in which arguments are generated, developed, expressed and reported. Each discipline, in short, can be seen to comprise a distinct culture, much - though not all - of which can be understood through a scrutiny of its own working language and the setting within which that language is generated.

Having at this point reached the limits of what I feel usefully able to say - the limits of my language, and hence the limits of my world - it seems fitting to close, as I opened, with one of Wittgenstein's aphorisms:

> "Whereof one cannot speak, thereof one must be silent."
> (Wittgenstein, 1922: Prop.7)

Coda

The specification for this conference paper stipulated that it must include "a critical review of Bob Clark's The Higher Education System". I had hoped, when I decided to tackle the theme of disciplinary discourse, to be able to apply some of my findings to his work. However, the attempt to characterise his writing in terms of its linguistic features has proved singularly unproductive. The approach adopted in my paper presupposes a particular disciplinary stance: but The Higher Education System is unashamedly interdisciplinary, ranging across history, sociology, anthropology, political science, organisation theory and other disciplines besides those. It adopts no specialist terminology, being written throughout in good, clear English prose. It is, in short, proof against the somewhat blunt tools employed in my analysis.

There is of course another critical perspective which can be brought to bear on Clark's book. Since it is itself a comprehensive study of the world of academia, one might enquire whether it has any observations to make about the ways in which the language of different disciplines throws light on their cultures and their epistemologies. In this particular respect, The Higher Education System is relatively disappointing. I have been able to find only two specific references to language. The first is on p. 29, where - early in his chapter on "Work" - Clark observes that

> Academic scientists, in particular, find it natural to practice world community. Their disciplinary perspectives and interests readily extend across nations, much as people in specialized lines of industry, commerce, and banking find more in common with counterpart specialists abroad who 'speak the language' than with others outside the specialty at home.

Again on p. 79, in his chapter on "Belief" he comments:

> everywhere at the core of the disciplinary subculture we find a 'common vocabulary', one inreasingly so arcane that outsiders find it mystifying and call it jargon. One way to investigate changes in the beliefs of a discipline is to track changes in vocabulary. The internal splitting of a field will be reflected in a differentiation of terms.

In the same chapter he writes thoughtfully and sensitively about "the culture of the discipline" - about distinctive intellectual tasks, knowledge-traditions and codes of conduct, shared intellectual beliefs and common paradigms, about heroes, admission and membership requirements and the rest. But linguistic usage (in the broad sense I have attempted to give it in my paper) is, I would want to argue, of central importance in defining any cultural grouping. Even though Clark sets out in his book to cover an ambitiously large area of territory, and cannot therefore dwell in detail on any particular theme, it is a pity that he does not do fuller justice to the role and implications of language in higher education.

It is, I acknowledge, a hazard of any wide-ranging interdisciplinary study that it is unlikely fully to satisfy those critics who choose to adopt a particular disciplinary stance. My own view is perhaps best summed up in the words of the historian from whose review I quoted in my paper: "the book is so good that one would wish it even better" - at least in respect of its treatment of disciplinary discourse.

NOTES

(1) I am indebted to John Swales for drawing my attention to a number of the sourse materials used in this paper.

(2) The very act of writing it is paradoxical, in the sense that a (long lapsed) linguistic philosopher has no valid claim to be a linguistician. Much of what is said is therefore at the margins of my competence. An appropriately qualified person would doubtless tell a different and more sophisticated tale.

(3) This choice has the advantage of taking into account three of the detailed studies within my own research (Becher, 1981).

(4) This is, of course, a point frequently made by sociologists of science.

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Tomas Gerholm:

THE ETHOS OF HIGHER EDUCATION SYSTEMS: A VIEW FROM ANTHROPOLOGY

Burton Clark's comparative study of the higher education system is, by all standards, an impressive achievement. Whatever one's discipline and whatever the angle from which one is trying to catch a glimpse of the multivarious creation that is the modern university (to forget about all its many varieties so well catalogued by the author), one can hardly do it without first reading for inspiration in Clark's book. Let the experts in one or the other of the subfields covered by Clark find fault with this or that; as a nowcomer to research on the higher education system I want to use his treatment of the comparative dimension as a stepping-stone. Can it be taken further? This is my question.

I.

But first a prelude. Among the various social scientists paying a brief visit to the field of higher education and then returning to their own concerns, Burton Clark (1983, p.1) fails to mention the anthropologists, and probably for good reasons. Not many have done more than pondered over their own territory of the academic universe and their own fences. Therefore it might be useful to discuss a little bit more in detail what an anthropological perspective on higher education might mean. Let me mention a few alternatives.

Anthropology is the study of the "others", however that concept be defined in each individual case. An anthropological perspective might entail viewing the inhabitants of the higher education system as really <u>other</u> people, fundamentally and spectacularly different from those we are familiar with from our everyday life, just as exotic as !Kung Bushmen, Eskimoes or Balinese. Exoticising the familiar is amusing, at least for a while, but it is a kind of estrangement that can only be a first step towards real insight. Anthropologists dealing with !Kung Bushmen, Eskimoes or Balinese do not, in fact, remain in the attitude of bewildered astonishment at the antics of the host population. Their work consists, rather, in reducing the immediately experienced strangeness of the others and translating it into familiarity. Taking the opposite direction, making the familiar into something exotic, may have an impact similar to that of Brechtian techniques of Verfremdung in theater. It may produce a frame of mind which dissolves the taken-for-granted attitude and thereby facilitates analysis. Thus, it can certainly be an initial device and a preparatory move, but not a permanent "perspective" providing orientation for a research project and even less, of course, a platform for a whole set of studies of the higher education system.

Another way of applying an anthropological perspective would be to emphasize the methods by which anthropological research is usually carried out, i.e. "field work". This is a catch-all term covering participant observation, informal interviews verging on casual conversation, formal surveys, hours and hours of questioning key informants, various technical means of "eliciting" information on principles of classification, semantic domains and cognitive structures, etc. At least the more informal components of this tool-kit I would regard as absolutely essential to the anthropological approach, and I cannot

conceive of an anthropological study of the higher education system that would manage completely without field work of some kind. On the other hand, field work is not synonymous with anthropology. Members of other academic tribes, sociologists for example, are also doing field work without for that matter changing skins and becoming anthropologists. There is more to a scientific approach than its methods. Those may migrate from one discipline to another. The same goes for theories. Innovation in one field is often a consequence of the borrowing of the ways and whims of another science.

More difficult to import is the style of a discipline's discourse, its way of posing questions and of answering them. Such a style is not mere embellishment, it may be an expression of the very essence of the discipline as an intellectual tradition. I am thinking of the kind of stories that anthropologists are fond of telling both among themselves and to the passing visitor. There is the story of the discovery of hidden correspondences among superficially heterogeneous phenomena suddenly revealing a clear pattern, the story of how seemingly irrational behavior turns out to be fully rational once you know the context, and the story both of the arbitrariness of your own seemingly solid world and the solidness of the others' apparently arbitrary worlds, to mention only the most common ones. Stories or thought patterns of this kind also have a strong claim on being part of the "anthropological approach" to the study of the higher education system.

The first-mentioned master plot of anthropology is a rather watered-down version of functional analysis. If functionalism once signified an attempt to explain the presence of a certain social phenomenon by the positive effect it was deemed to have on the reproduction of the social system, it has by now come to stand for a much less ambitious venture. What remains is a curiosity about the interrelation of social phenomena, the many subtle ways in which they affect each other. The professed aim is not to explain how an entire social system is being maintained but, more modestly, to look for certain family resemblances between many of its features and, if possible, to account for their interrelations.

Such a way of thinking about the higher education system would make us look for more subtle connections than those of how, for instance, the university serves as a recruitment agency for various high offices in society or how the university is affected by manifest political programs in the wider society. There is nothing wrong with that sort of research, but the anthropologist would be more interested in finding the less commonplace and the more surprising connection. He or she might be looking for effects of the higher education system in unexpected parts of the social body, influences that have not been calculated with and that may be beyond a simple calculus in terms of positive and negative effects. I am thinking of such phenomena as the migration of thought patterns from the academic ambience to other social spheres, or the creation of ideologies, belief systems or world views that are exported without license from the university and for some reason or other adopted - suitably amended - in a totally different social setting. There is another way of thinking deeply ingrained in anthropology that might have fruitful applications in the study of the higher education system. It is a special variation on the general theme of contextual understanding mentioned above. It takes the form of a scenario that we may call "Central Initiative and Local Response". Anthropological literature is replete with tales of centrally planned administrative reform or development projects that miscarry because the relevant local conditions were not appreciated by the

planners. Faced with any attempt at planned social change, the anthropologist suspects, almost a priori, that the outcome will be a surprise, in one way or another, to the planners. Being more at home among the powerless than among the powerholders, the anthropologist has come to share the sceptical attitude towards the "central bureaucrats" which is often characteristic of the people at the grass-roots level of society.

But the position of the anthropologist was and is deeply ambiguous. As a spin-off effect of imperialism, anthropological research was always dependent on a position of superior power: anthropologists were members of conquering societies studying members of conquered societies. Their research was made possible, in the last instance, by this very power relation. On the other hand, they usually took the standpoint of "their" people siding with them against the powerholders, at least to the extent that they tried to represent the local point of view. But since the anthropological version of the local perspective was addressed to their own kind, to the conquerors, it was inevitable that, sooner or later, the anthropologists would be charged with delivering information to be used by the powerholders.

This basic pattern and fundamental irony of anthropological work is likely to be repeated and experienced by any anthropologist studying the "others" of the higher education system on behalf of - funded by, or at least read by - persons at the commanding heights of that system. But some of the drama is taken out of the plot. The "local" in "local response" may be everything from the university, as a component of the overall system, to the department, the teachers/researchers or the students. All of them are grass roots in relation to the next higher level in the system. The power dimension is also somewhat attenuated in this case by the fact that, although the levels of the higher education system can be clear-cut, the same persons may reappear at several of these levels as lower-level representatives.

The third master plot in anthropology is the general theme - with many variations - that we live in worlds of our own making. The sense of facticity and self-evident solidity that is, perhaps, the foremost characteristic of our common-sense world is a result of the precarious collaboration of many individuals taking more or less the same things for granted. "If men define situations as real, they will be real in their consequences," runs one of the better known proverbs of social science (formulated initially by W.I. Thomas). This applies both to the great entities we call "civilizations" or "cultures" and to such small-scale phenomena as occupations, disciplines and departments. The smaller they are, the more precarious are their realities and the more important the role of the individual.

The university is an institution producing not only diplomas and graduates but also <u>images</u>: images of itself, of the world and of its own place in the world. These images differ from those produced by other institutions in society, and they also differ within the university itself from department to department, from discipline to discipline. To borrow an expression from Nelson Goodman (1978), the university contains many ways of "world-making". As Clifford Geertz (1983, p. 155) has reminded us,

> /.../ the various disciplines /.../ that make up the scattered discourse of modern scholarship are more than just intellectual coins of vantage but are <u>ways of being in the world</u>, to invoke a Heideggerian formula, forms of life, to use a Wittgensteinian, or varieties of noetic experience, to

> adapt a Jamesian. In the same way that Papuans or Amazonians inhabit the world they imagine, so do high energy physicists or historians of the Mediterranean in the age of Philip II - or so, at least, an anthropologist imagines. (My italics).

An anthropological perspective on the higher education system might mean several things, as we have seen, so this is not the perspective, only one that in my view is full of challenge and, perhaps, promise.

What I want to do in this paper, however, is something slightly different, although it also has to do with the social construction of reality, this close and fateful relation between definitions and actions, between semantics and ontology. Just as the university is a producer of images of the world, every society produces images of the place of the university in society, ideas of the uses of the university and its relations to the central concerns and values of society and its various groups and classes. Some of these notions, no doubt, originate in the university or at least with people who have a university background. The social origins of these various images of the university is essential to the understanding of how the university has developed in different countries, as shown by Ben-David and Zloczower (1962). I shall return to this issue in connection with a discussion of the place of the modern university. But before doing so I am going to have recourse to the most common of the anthropologists' devices when discussing phenomena in their own societies, i.e. comparing them to similar phenomena in other societies. "Other societies are good to think with," as Hannerz (1983, p. 33) has said.

II.

The modern university is not more than perhaps 150 years old, but institutions of higher education can be found in all complex civilizations. I shall try to situate the university in modern society by comparing it to two other institutions of higher learning, the Jewish yeshiva in the shtetl, the Eastern European market town, and the Islamic madrasa in "traditional" (i.e. preprotectorate) Morocco. My comparison concerns their respective places in society, their relation to the "center" or "centers" of society. In order to proceed we need, therefore, a clearer view of what is meant by the notion of a center of society. Here I follow Edward Shils who has elaborated this idea in several publications.

The notion of a center of society is an aspect of its integration, of what, in fact, makes it a society. The center is more than a place "where decisions are made and coordinating functions are performed" (Shils 1975a, p. xxxii). It has "nothing to do with geometry and little with geography" (Shils 1975b, p. 3). Although, as we shall see, there is a relation both to political authority and spatially located administrative centers, the center of society is a notion referring to the values and imaginary constructs of a society.

> To be a society, a social system must have its 'center of gravity' within itself, i.e., it must have its own system of authority within its own boundaries. It must also have its own culture. Part of its culture it will necessarily share with other societies from which it derives and with which it has intercourse - but part of this culture will be particular to itself. Some of this particular culture will be about itself. It will consist of beliefs about the history and nature of the society, its relationship to certain ideal

> or transcendent entities or values, its origin and destiny. It will include beliefs about the rightfulness of its existence as a society and about what qualifies its members to belong to the society. It will, of course, include works of art, literature, and thought - many of which will reflect on the just-mentioned topics. (Shils 1975c, pp. 36-37).

To be a member of a society implies a consciousness of society and this consciousness can never do without a certain notion of the "center of gravity" of that society: this is what Shils is saying. His argument certainly implies a consensus model of society, but it is a consensus of a very minimal range, not really vulnerable to the many closely argued criticisms that have been directed against such models. He explicitly acknowledges that centrality and the authority associated with it are relative notions and that their normative weight is not equally heavy in all parts of society. He is also aware that societies may have alternative or rivalling centers and that the centers of some societies may impose themselves even on other societies. This has been characteristic, throughout much of modern European history of the relation of Paris and French civilization on the one hand and much of European society on the other (Shils 1975c, p. 39), but it is also a characteristic feature of colonialism. In fact, Shils's first observations of "centrality" were made in India (Shils, 1975a, p. xxxi). It was the way Indian intellectuals tended to look towards London and what it symbolized that made him think in terms of centers and peripheries. Later he found that the behavior of the intellectuals was only an extreme form of an orientation that is common to all members of society.

As already indicated, there is a close relation between the focal points of a society's value system and its central institutions. The values are not freely floating but anchored in these institutions. This is one reason why the center of a society, in Shils's sense, very often also has a spatial location. There is

> a spatial center /.../ which is, or is thought to be, the seat of the central institutional and cultural systems. To this center or centers, much of the population looks for guidance, instruction, and commands concerning conduct, style, and belief (Shils 1975c, p. 39).

The question I want to discuss, then, is the relation of the higher education system to the center of society, and I propose to do that by first outlining the corresponding relations of the yeshiva and the madrasa to their respective centers of society.

III.

There is an Islamic concept referring to both Jews and Christians as ahl al-kitab, "People of the Book", i.e. people who had received the divine revelation in the form of holy scriptures before the Arabs did so. In the case of the Jews, the epithet is singularly apposite: no people has been so dependent on a book (and on books about the Book) for its very survival as a people. When the Jews lost their political autonomy to Rome and were forced into exile, "The Book became the portable homeland," as someone has put it (Maller 1961, p. 1234). In the Diaspora, "survival was an act of interpretation," as another one has said (Faur 1983, p. 61). Ever since its completion in the early sixth century (A.D.), the Talmud has constituted "the principal, formative element in the life of the Jewish people" (Neusner 1979,

p. 108). Nowhere was this truer than in Eastern Europe where the majority of the Jews lived scattered in small-town settlements among largely hostile people who time and again subjected them to terrible pogroms. If they were able to maintain their identity, this feat was mainly due to their extraordinary efforts in educating themselves about who they were.

The Jewish communities were islands of literacy in a vast sea of illiteracy. As Mark Zborowski and Elisabeth Herzog, two pioneers in the now fashionable field of historical anthropology, wrote in their classic Life Is With People: The Culture of the Shtetl:

> A Jewish community without a center of learning is unthinkable, and a shtetl of any size will have several varieties of school: a kheyder, where the youngest children study, a besmedresh for both prayers and study, a talmud toyreh for those whose parents cannot pay tuition, a yeshiva for higher studies. Study is the duty and joy of a man reared in the shtetl tradition. As a duty it is twice prescribed. In order to be a good Jew one must obey the commandments of the Scriptures. In order to obey, one must know them. In order to know them, one must study. Moreover, to study is a mitsva, a deed commanded by God. (Zborowski and Herzog 1965, p. 71).

What did they study? In traditional shtetl society mainly the Book (the Pentateuch) and its commentaries (the Talmud). Secular subjects were kept out of the education system of the shtetl. The curriculum of the first stages of the kheyder provided the basic layer of the ethnic consciousness:

> The peculiar position of the Jews as a chosen people, the inherently mythic distinction between them and the nations, an understanding of the fate of the Jewish people in the Diaspora and their faith in their coming redemption /.../. (Katz 1961, p. 190).

The fundamental aim of the whole system of education, of which the kheyder constituted only the very first step, was to produce scholars who could, as it were, study the Law for the people. "A thorough knowledge of and preoccupation with the Talmud, although acquired only by a minority, constituted a value of supreme importance to society as a whole." (Katz 1961, p. 191). Those who could not devote themselves to the study of the Law had other ways of approximating to the ideal, for example by economically supporting either the institutions of learning in general or a particular scholar. "An entire people lived for scholarship; each, according to his own means, studied the Law." (Menes 1961, pp. 383-384).

On the highest level these studies were carried out in the yeshiva, an institute preparing for the rabbinate but in fact having a majority of students who did not aspire to be rabbis but were there to gain knowledge for its own sake or, as it were "serve God through the continuing study of his Revelation and through the making of that study into the motive and definition of the purpose and meaning of Israelite life" (Neusner 1979, p. 119). There have been many denigrating remarks about the character of those studies, the "hairsplitting", the "juggling of categories" etc. - in fact a feature of the antisemitic portrait of the "Talmudic Jew" not to be overlooked - but the insider is more likely to stress the intense intellectual satis-

faction that Talmudic scholarship had to offer:

> The Talmud is notable for allowing the student many possibilities for searching and for thinking. Herein lies a basic difference between the Bible and the Talmud. In studying the Bible the student is confronted with the word of God; 'the written Law' thus does not admit any discussion, only differences of interpretation. The Talmud - 'the oral Law' - on the other hand, confronts the student with human beings - great men, to be sure, Tannaim and Amoraim, but only men. The Talmud not only enables the student to interpret the Law but also to introduce innovations. The creators of the Talmud brought the Law down from heaven to earth, and presented it to man. The fact that the Talmud offers many differences of opinion has been particularly exciting to the imaginations of young students, for this meant that one could choose between one opinion and another, and, furthermore, that everyone had a chance to be creative. (Menes 1961, p. 386).

Although knowledge of the Law was basically an instrument of the righteous life, it became among these students something sought for itself. There was no need to look for possibilities of converting scholarship for something else, using it to get into other spheres of society, translating it into high office or money, if that had been possible. To study was not a means to reach another station in life. It was, already, to be at the very heart of Jewish society.

Considering how this study-as-worship helped maintain Jewish identity, one is tempted to concur with Emile Durkheim's (1912) general interpretation of religion as a group's (unconscious) worship of itself. No supporting case could be more convincing than the Jewish one.

IV.

"The Prophet regarded any day as lost in which he did not increase in that knowledge that would draw him closer to his Lord." In these words of al-Ghazzali (quoted in von Grunebaum 1969, p. 11), one of the foremost theologians of Islam of all times, we find an expression of the Muslim attitude to study. al-Ghazzali is referring to such knowledge as will make it possible to follow God's will, obedience being man's purpose on earth. This is a general, religious conception of knowledge, common also to the sister religions of Islam, Judaism and Christianity.

In the case of Islam, the quest for knowledge of Him and His Law led, first of all, to the study of the Quran which is regarded as the literal word of God. But in addition, the life of Muhammad was considered to be a guide to the correct life in all its aspects. Therefore it became important to know exactly how the Prophet had behaved not only in worship but in all the situations of everyday life. What the prophet had done and said on various occasions thus became a second source of knowledge of how to serve God. In order to prevent the appearance of inauthentic traditions, methods of judging their reliability were developed. And in order to correctly understand the Divine Revelation, the very text of the Quran was analysed in a way that gradually gave rise to the sciences of grammar and philology.

From these two sources, the word and the life, grew the central body

of knowledge on how to lead an Islamic life, the shari'a the Canonic Law. From these two sources grew also the conception of the Islamic state. Its purpose was to make a life in accordance with Islamic principles possible by providing the institutions necessary to enforce the stipulations of the shari'a and to protect the Muslim community against both internal and external enemies. Interpreted and extended by analogical reasoning, the Divine Revelation served as a blueprint for Muslim society. In this respect, there are striking similarities between the Jewish and the Islamic religions. Muslim society became just as dependent as Jewish society on the existence of a body of experts on the sacred law. It is the education of these men, the ulama, that concerns us here.

Before looking at the educational institutions themselves, a few words need to be added on the Muslim attitude to knowledge. We are concerned here with the knowledge that mattered, the knowledge that was necessary to lead a Muslim life, not the kind of knowledge that one could pick up in everyday life, common or secular knowledge that was not dependent on the divine revelation. The "necessary knowledge" (al-Ghazzali) was conceived of as both finite and normative. The Quran being the last revelation to mankind and Muhammad the last of the prophets, what God had to say had already been said. Now it was the task of the learned to hand on this knowledge, as exactly as possible, to new generations. When, after the first centuries of unparalleled expansion and development of Muslim civilization, the first signs of decline began to show, the conservative embryo inherent in this conception of knowledge grew into fullblown pessimism. Later generations were expected to be on a lower level both intellectually and morally and the best one could hope for was an authentic transmission of the revealed knowledge (Hodgson 1974, p. 438). Just as the early efforts to guarantee the authenticity of sayings and doings attributed to the Prophet relied on a chain of trustworthy persons leading all the way back to the contemporary witnesses of the Prophet, so the acquiring of the knowledge of the religious sciences came to depend on the correct transmission of the thoughts of a few accepted authorities. The "door of independent inquiry" had, in principle, been closed. Even though enterprising minds, of course, did find ways to get past the barred entrance, the official encouragement of faithful repetition and ban on originality marked the intellectual climate among learned men. When the institutions of higher education in the Muslim world were born around the year 1000, this attitude to the Islamic sciences was already fully formed. From the very beginning they became places for the simple transfer of fixed knowledge from one generation to the next (Waardenburg 1965, p. 135).

To make all this a bit more concrete, let us look at one such institute of Muslim learning, the Qarawiyin mosque in Fez, the undisputable intellectual center of pre-protectorate Morocco. A hundred years ago it still functioned as it had done in the Middle Ages, although the country's growing economic links with Europe and the political threat from the French colonization of Algeria had just begun to unsettle traditional Moroccan society.

The purpose of the education at the Qarawiyin mosque was to provide the believers with "une connaissance aussi parfaite que possible des vérités a croire et de la ligne de conduite a tenir en fonction de ces vérités" (Le Tourneau 1965, p. 152). If sciences without an immediate relation to these truths, such as grammar, logic and geometry, were taught as well, it was because they were considered to be of help to the student in his essential endeavors. They had no intrinsic value.

These were the subjects taught in the mosque-university at the turn of the century:

> la tradition prophétique ou hadith, traitée tous les jours pendant trois mois de l'année; les sources du droit, enseignement donné l'après-midi, c'est-à-dire tenu pour secondaire; le droit ou fiqh, considerée comme la science fondamentale, celle sans laquelle on ne puvait etre regardé comme un homme cultivé; la grammaire, enseignée l'après-midi d'après des livres fort anciens, dont un rédigé en vers; la rhétorique, enseignée par de grands professeurs; la logique, peu en vogue a Fès; la prosodie, enseignement donné seulement les jeudis et vendredis; le calcul, professé en dehors de la mosquée des Kairouanais /Qarawiyin/, dans une mosquée de quartier; l'unité de Dieu, c'est-à-dire la dogmatique; la pratique juridique, considérée comme importante; et enfin la littérature, qui était limitée a l'étude de poemes anciens en l'honneur du Prophete et de leur commentaire. (Le Tourneau 1965, pp. 152-153).

There was no conception of an academic year, but most new students arrived after the harvest. One could begin at any time, there was no need of registering and no fees to be paid. One simply presented oneself to the professors whose courses one wished to follow. The studies usually lasted for at least five years, but to master all the essential texts might take more than double that time, since "mastering" often required as a precondition that they had been learnt by heart. There were no exams and generally no control of what the students had learnt except in matters of recitation where mistakes would always be corrected. On leaving the Qarawiyin a student could get a kind of certificate from his teacher or teachers that he had followed such and such a course (which consisted in reading authoritative books: lectures were always commentaries on the meaning of books read together). The certificate (ijaza) per se was not important, it was the teacher who had issued it that mattered.

To become a teacher himself, the student needed only a certificate from his teacher authorizing him to teach what he had learnt from that very teacher and indicating in a rather general way the subjects covered. The new teacher was then free to start teaching his course wherever there was a free spot in the mosque, in the beginning totally without remuneration. After a trial period he could apply to the sultan to be employed as a regular teacher. As such he now belonged to a diffuse category of learned men, the ulama, making up the religious (and therefore also the administrative) elite of the country. But even without a teaching position at the mosque university a man could be recognized as a learned man and counted as a member of the ulama. Such a status was a matter of general reputation for learning rather than something that could be achieved by meeting formal requirements. As a matter of fact, not all the teachers at the Qarawiyin were always included. It was quite normal, on the other hand, that individuals outside the mosque university - such as judges, prominent descendants of the Prophet, heads of Sufi brotherhoods as well as persons who were well-known for their piety and learning - were regarded as members of the ulama (Burke 1972, p. 101).

The ulama did not constitute a corporate group in Morocco, but rather a general social category. It was contextually defined, its actual membership could vary from occasion to occasion and even from the perspective of one individual to the next. To understand the nature of this category one does well not to think in terms of the Christian

clergy or the Jewish rabbinate. One should compare, rather, with the notion of "intellectuals" in contemporary Western society, a category also found both within and outside the university and displaying qualities that do not necessarily come with the passing of academic exams.

In spite of this looseness of organization, the ulama played a considerable role in the functioning of the Moroccan state. The sultan's legitimacy was derived both from his descendance from the Prophet and from the consent of the scholarly community to his accession to office. Asking the consent of the ulama was a symbolic gesture on part of the sultan-to-be and it may have been more important to the learned king-makers than to the ruler, but in highlights the symbiotic relation between the ulama and the sultan, between knowledge and power, as Kenneth Brown (1972, pp. 139-140) has emphasized:

> The relationship between government and ulama was one of mutual support which expressed itself in various /.../ symbolic and real forms: frequent official delegations of the ulama to the sultan; visits and demonstrations of respect by the sultan or highranking members of his entourage to well-known scholars; requests by the sultan for advice and judicial opinions; the entrusting of young members of the royal family and retinue to the ulama for the purposes of education; the appointment of ulama to religious, educational, and administrative posts.

In a society "saturated by theological considerations and values" (Brown 1972, p. 140), such regular exchanges with the rulers could only confirm the central position of the ulama.

To attend the lectures in the Qarawiyin mosque could be an avenue leading into these circles of prestige, influence and power. In the fluid society of Morocco it was possible - even if not easily done - to move from a place in the social periphery to a position close to the social and cultural center.

Religious learning could be a means to social advancement also in another way. The Qarawiyin was, above all, the place where the elite families of Fez sent their sons to acquire the education and the manners that suited their station in life. It was here that they acquired some of the "cultural capital" which made them fit to be the undisputable leaders of their city. It was also here that many lasting ties, building-blocks for networks useful in the future, were forged. In a study of the Yusufiya, the Marrakesh equivalent of the Qarawiyin in Fez, Dale Eickelman (1978, p. 507) has shown that such calculations were not uncommon among those who entered these centers of higher education:

> In discussing their years at the mosque-university, most emphasized the opportunities they created to secure ties with persons within and outside the community of learning. These ties often were of use later in facilitating commercial, political and entrepreneurial activities. The frequency with which such ties were mentioned suggests an implicitly shared conception of career, although not in the narrowly occupational sense of the term.

To attend the Qarawiyin, the Yusufiya or the various madrasas of the smaller centers of learning in 19th century Morocco was, thus, not necessarily to embark on a "religious" career in any narrow sense of

the word. As we have seen, such studies could lead to any one of a plethora of occupations, some of them securely anchored in the midst of the comfortable elite of the country. Before the encroachment of the French had begun to challenge the self-image of this elite, the mosque universities were centers of all the learning that was worth having, thus places of extreme confidence. This sense of place, at the center of society, has been well captured by Jacques Berque (1949, p. 107, here quoted in Kenneth Brown's /1972, p. 127/ translation):

> The scholar walks along the street, his eyes lowered, his prayer rug under his arm. Unctuous, his step expresses disdain for the sights of the world about him. Rather it seeks and obtains the advantageous veneration of the masses. This devoted teacher has been attached always, by a thousand-year tradition, to the affairs of the city. Only rarely has he given himself over to brutal asceticism or ill-bred pedantry. The master of religious science is at the same time the master of the right tone. His slippery courtesy, his nerves of old city dwellers, his ruse of an old courtier, his attachment to ornamental velvets and the sculpted plasters of magnificent houses, make him the intellectual champion of a culture that expresses itself equally well by the erudition of the scholastic, the fine hand of the craftsman, or the succulence of its cuisine.

V.

With these thumbnail sketches of higher education in two very different societies I have reached the end of the ethnographic part of this paper. There is no need to try to make an equivalent sketch of higher education in Euro-American society: we already have a finely worked portrait in the book that is the raison d'etre of this conference. The use of these excursions to preprotectorate Morocco and pre-World-War-I Eastern Europe, if there is one, would be to help us take a new look at our own madrasas and yeshivas to see what sort of places they "really" are. An implicit comparison of this sort may serve to bring out features of the higher education system in modern society that we might not think of otherwise, or at least to help us frame new questions to be addressed in researh.

In a sense, then, the paper begins here, with the few points that my own thinking about these very different institutions of higher learning have led to. But in another sense, this paper can hardly do without the ethnographic preliminaries. It tries to give a very tentative demonstration of a way of thinking comparatively about Western institutions. This is the essential. The points that follow are made just as indications of what kind of questions such a comparative approach might try to elucidate.

1. In these accounts both Jewish Eastern Europe and traditional Morocco emerge as monolithic societies having one center, and the higher education system is located at this very center. It seems no exaggeration to say that the yeshiva and the madrasa are the most fundamental institutions in their respective societies, if we are to go by the official cultural self-image. Would it be possible to make the same claim for the position of the university in Euro-American society? Or is it the case that modern society is so complex that it makes no sense of speaking of it as having one center? Perhaps the higher education system itself is a federation (to use Clark's apt

expression) of fundamentally different units that stand in fundamentally different relations to whatever center or centers there may be? By means of these questions I am arguing for an analysis of the "meaning" of the university, the cultural notions surrounding the university in our society or, in other words, the university as a cultural construct.

2. The yeshiva, the madrasa and the university are all houses of knowledge, places where knowledge is being managed, handled, and attended to in various ways. The conception of knowledge may differ from house to house - in the case of the university certainly even from room to room - and the culturally appropriate attitude to knowledge will differ too. To strive for knowledge in the yeshiva is in itself to serve God, in other words the highest cultural value. To strive for knowledge in the madrasa is to serve God but it is also to enter the culturally hegemonic elite in a society infinitely more complex than that of the shtetl and even to have a say in the affairs of the state. The connection between knowledge and power or, perhaps better, between knowledge and authority is here very evident. To strive for knowledge in the university is ... We immediately realize the complexities involved even in finishing the sentence! To begin unravelling these complexities one would have to start by analyzing the various meanings of knowledge. In the two religious systems of higher education that we have been discussing, it is self-evident that the only knowledge worth bothering about in the house of learning is knowledge of the divine revelation and the various disciplines that man has developed in order to understand that revelation and its implications. Especially in the Islamic case there are elaborate classifications of the branches of knowledge and their respective value. Knowledge is hierarchical. This is not the case in the modern Euro-American system of higher education where there is at least an official ideology of the equal importance of all subjects, even though there may also be a widely felt suspicion that some are more equal than others. How would one go about discovering the hierarchies of knowledge in Western society? Perhaps by embarking on an extensive "ethnography of knowledge" while keeping in mind that there may be complex relations between locally defined hierarchies of knowledge and conceptions elaborated by politicans, experts, lobbyists and intellectuals: in short, by people like us.

3. Such an ethnographic, i.e. descriptive, undertaking might be guided by some of the different conceptions of knowledge emerging from our visits to the yeshiva and the madrasa. "Originality" is a notion that does not seem to belong in those two houses of knowledge, least of all in the madrasa. The emphasis is on the accurate transfer of a fixed body of knowledge. The ulama show their "originality" mainly in their clever application of the knowledge stored to the case in point which, in a way, always is unique. The yeshiva students have a larger area where "originality" is culturally rewarded: not only in applying the Law but also in comparing, criticizing and perhaps superseding the commentaries of their predecessors. In the modern university the official, idelological emphasis is on "originality", the creation of new knowledge. This is how we, as academics, usually understand and evaluate our work and that of others. But this is a notion that may vary somewhat from discipline to discipline, perhaps sometimes even from person to person. If we all pay our respect to the "production of knowledge" - a key metaphor of the modern factory of knowledge - it may be that somewhere within us there is a yeshiva student or a member of the ulama lurking. Modern notions of the idea of the university as the home of scientists bent on moving the frontiers, increasing the House of the Known, building new verandas, adding a floor or two,

should not blind us to the fact that the university may also be the home of a deep respect for tradition, a feeling that the most urgent task is to pass on insights, morals and devotions of the past to those who come after us.

4. Returning to the notion of a center of society, let us approach it from a different angle. To put it bluntly, what are the "uses" of knowledge? What can it be exchanged for? Anthropologists are familiar with scienties in which there are various types of values that, as it were, move in separate spheres: yams and cereals; slaves, cattle and metal rods, rights in women. Traditionally, objects within one of these three spheres could not be exchanged for objects in another sphere. However much yams you had, you could never "buy" a woman for it: a woman could be had only in exchange for another woman. But then, the story goes, came Western money to the village and soon established itself as a medium of exchange in one sphere after the other. The old hierarchy of differently valued spheres of exchange broke down. How the story would go in our case if we regarded the knowledge of the university as a general means of exchange, I do not know. In the shtetl, certainly, knowledge could be used to obtain anything that was at all available in that society (but chances are that the learned were not all that interested). In the Muslim society of Morocco, knowledge also served as a passport to all parts of the social body. In modern society, however, we seem to have a case of the creation of impenetrable spheres. Only equipped with a certain kind of knowledge will you be let in. In contrast to Western "general-purpose money", there is only "special-purpose knowledge". The nature and growth of this society of compartmentalized knowledge might be another subject for the anthropologist who has tired of the yams, the cattle, and the women.

Being such an anthropologist I shall end here by stating explicitly what I have been saying implicitly all through this paper. If anthropologists have a contribution to make to the field of higher education research it will probably take the form of unpacking the cultural content of the idea and reality of the university. The anthropologist will deconstruct it locating the factory, certainly, but also finding the hidden entrance to the madrasa and the steps leading to the yeshiva.

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Sven-Eric Liedman:

INTEGRATION AND HIGHER EDUCATION

Burton Clark's The Higher Education System is an extremely useful study. Here, Clark summarizes the main results reached in the field of higher education studies during the last decade, and here he ventures to formulate his own ideals and to sketch his own university utopia. His perspective is broadly comparative with an unmistakable US accent: the American system is to him the normal one, to which all the others are related as exceptions from the rule.

It may be tempting to contrast Clark's results to those presented in studies belonging to totally different intellectual traditions. I have in mind e.g. Pierre Boudieus homo academicus (1), which with its stress on conflicts inside the university system (le conflit des facultés(2)) would offer an effective contrast to Clark's fundamentally harmonious interpretation.

In this essay, I will just hint at such a comparison, my main subject being to call into question some of the open or hidden essential assumptions of Clark about the nature of scientific (3) development and about the relationship between university and society. Before presenting my argument, I will just sketch my own background and professional identity, which, I am sure, differs substantially from that of Professor Clark.

There are two types of assumptions in Clark's study that I want to call attention to, namely:

(i) that the process of specialization and diversification of science and of the system of higher education is irreversible and irresistible; and

(ii) that the system of higher education first of all must be studied in an "internalist" perspective, its main relations to the surrounding society being those to the political authorities and/or to the market in a nonspecified sense.

In my discussion of these points, I shall provide examples from Clark's book. First, I am turning to my own background and my research interests.

1. History of science and the process of specialization

In Sweden, there is a discipline called idé- och lärdomshistoria (literally the history of ideas and erudition of learning, normally translated as the history of ideas and science). Already the name tells us that its scope must be very comprehensive. In the Anglosaxon world, its counterparts are not only intellectual history and history of science but also substantial parts of philosophy, politology etc. Typically, a Swedish historian of ideas and science, when studying universities, is not only interested in the university system as such. He tries to put this system in connection with both the position of university in society as a whole and with the knowledge produced

inside the university. To him, it is an important task to combine internalist and externalist perspectives.

Considering the broad perspective of the discipline, it may be seen as quite natural that the mechanisms of scientific specialization since long have been one of my main research interests. Even in the history of science, the process of specialization is normally seen as a "natural" process. Now, such commonsensical opinions - that in fact are much more frequent in scientific reasoning than we normally realize - must be treated with skepticism. Why would specialization be inevitable? In most cases, the explanations are not even worked out but only implied. The bulk on knowledge is increasing, somebody hints without explaining if to be a specialist really means to have all the details of a certain field of knowledge present to one's mind. Scientific specialization is just the other side of the ongoing division of labour and tasks in society, another saying suggests. But how is science influenced by social development, and what is the impetus of the social division of labour?

First of all, what <u>is</u> a scientific specialty? Nowadays, "specialty" and "profession" are often used synonymously (4), a circumstance which I find highly inappropriate. A profession must be institutionalized, the real professionals having, thanks to their training, their examination and their position, a real monopoly of their field of work. Adam Smith was not professionally an economist, but he was in any reasonable sense of the word a specialist. Doctors are professionals but represent a lot of specialities and even some non-specialities (e.g., social medicin).

In order to understand what a scientific specialty is like, we have to look at a non-specialized academic culture. We may say that the 17th century university was highly non-specialized. This does not mean that there was no division of work at all there. The theologians had their duties as scholars that were definitely separated from those of the natural philosopher or the jurist. Still, they were normally no specialists. And why? Their field of competence was not close and clear, the theologicans having always the duty to look after e.g. that the doctrines of the natural philosophers did not make any harm to Christian faith. This is the same as to say that the fields of work did not concide with the fields of competence, the latter being substantially larger in the case of the theologians, narrower in the case of the natural philosophers. This is typical of every hierarchical work structure, even in the modern enterprise.

The concepts of <u>field of work</u> and <u>field of competence</u> are both, I will argue, necessary in order to define a specialty. Specialization is not only the case of division of labour. The workers in Adam Smith's famous example got their different tasks in the pin production not as specialists; on the contrary, according to Smith they turned out to be specialists after having a lot of training in their limited field of work.

In short, in a true specialty the field of work coincides (or nearly coincides) with the field of competence. If a specialist, the range of your job tasks is limited by the competence you have due to your training or your former work experiences. The main point is not that the scope of a specialty is narrow but that it is definitely limited. On the other side, it may be a sound empirical hypothesis that the narrower the field of work, the more specialized it tends to be.

Specialty then is defined as a composite concept. It has an inner tension between field of work and field of competence. Thence, it is an efficient tool to handle the dynamic process of scientific specialization.

Which are the main forces behind the process of scientific specialization? - Here, I may just hint at some bold hypotheses, the most important one being that science is fundamentally determined by its use in society. (By this hypothesis I am not arguing that science can only have an "instrumentalist" value; on the contrary, as will soon be clear, my concept of use is very broad and includes what is normally called "knowledge for its own sake", which in fact is a certain use of knowledge.) I make a distinction between three kinds of uses: material, administrative, and ideological. Material use of science means that science effects the material productivity in society; due to science, we can produce more and produce otherwise unseen products. Material use is typical of "useful natural science". Scientific knowledge may also play a part in developing administrative, political, or legal organization, arrangements and rules. Here, science is engaged in social and political engineering in the broadest sense of the word. I prefer to say that science here has an administrative use.

The third kind of usefulness is nowadays easily overlooked. Still, its historical importance is overwhelming, and without doubt it will influence scientific development forever. Science is used in order to alter, to strengthen or to corroborate the opinions, attitudes, values etc. held by different individuals, groups and classes in society. In our example of the theologians watching over natural philosophy, we can see a typical example of an ideological use of science. Natural science had first of all to instruct people about the furniture of this world, and their discription turned out to be dangerous if not compatible with the Christian doctrines. Now, even the opposition represented by Galileo or, later, Buffon, delivered material for ideological use, i.e. for the construction of a new or alternative world view. Einstein did not only stimulate the development of the atom bomb (without doubt a material use of science!) but he also delivered a lot of arguments for the ideological debate of his time. In fact, such disciplines as cosmology, general sociology, history, philosophy, or literature, have substantially a pure ideological usefulness, even if their representatives are not always aware of this fact and then striving for other kinds of utility, if not arguing that they are striving for knowledge for its own sake, which in fact is an ideological use of it.

In what ways do then the different uses of science influence scientific specialization or integration? My hypotheses - which I have put to the test in a series of studies available only in Swedish or German (9) are, briefly, that ideological use tends to broaden the perspective and counteract specialization, whereas material or administrative uses favour further division of the fields of competence and work. This is, of course, a very complex process, the dynamics of which I cannot even touch upon here. Let me still make my argument somewhat clearer by giving a few examples.

In the 19th century, organic chemistry developed into a new specialty separated from inorganic chemistry. This process of specialization is intimately connected with the new and unforeseen usefulness of organic chemistry. For the sake of theoretical respectability, the chemists tried to develop a special theory for organic compounds, but in vain. It was the usefulness in agriculture and, especially, chemical industry that determined the boundaries of the new specialty.

At approximately the same time, Darwin's theory of natural selection scored its overwhelming success not only and not even primarily among biologists but in the broad audience, too. Materially, the theory was for a long time absolutely useless, not a single turnip growing faster because of the new knowledge about the mechanisms of natural selection. Ideologically, however, it was of overwhelming interest, not only challenging the creation myths of the Bible but also delivering a lot of inflammable material to the ongoing policial battles in which liberals, conservatives and socialists were taking part. The ideological interest that the theory caused forced it literally out over its own boundaries. Darwinian theory - and with it biology - had to answer questions about the place of man in nature, about man's historical development, and about the origin of ethics and religion. For some decades, Darwinism meant a kind of efficient scientific integration(6).

In the much more specialized scientific culture of the late 20th century, a possible counterpart of Darwinism would meet substantially greater difficulties in integrating such an immense scientific field. Still, we have more limited phenomena of the same kind as soon as scientific theories or conceptions turn out to be ideologically controversial. The case of sociobiology - in itself an effort to integrate already separated scientific domains - may be mentioned, just as the monetary theory in economics with its claims to form a basis for human relations in general. Broad perspectives spread over many disciplines such as Marxism or hermeneutics may to some extent have a similar, at least partially integrative effect.

Now, it is time to turn back to our main line of argumentation. One of the consequences of my approach to this complex of problems is that the organizational arrangements of scientific institutions must not be seen as a main cause of specialization. It is impossible to institutionalize a specialty if the main prerequisites for specialization are absent. Organization may make specialization easier, or hamper it(7).

One is also on the wrong track when supposing that a specialty is the inevitable result of peoples' striving for establishing new niches for themselves (8). This tendency may be an important subsidiary factor behind specialization, but just as all explanations starting from assumptions about human nature or constant social relations, it cannot explain a phenomenon that is not universal but only developing in certain milieus and during certain historical epochs.

2. Clark on specialization

A very central general thesis in Professor Clark's study is about the irresistibility of scientific specialization and, implicitly, about its inner relationship to the division of labour in society. Let me quote a typical line:

> No one has yet found a way to slow the division of labor in society. No one is about to find a way to stop the division of knowledge in academic society (9).

Or, again, still more generally: "Growth has led to division"(10). On this point, Clark seems to be heavily influenced by Emile Durkheim's De la division du travail social and the tradition starting with this book. According to Durkheim (and, in fact, to Darwin, from whom Durkheim got his idea) competition tends to get harder if the competitors

are similar to each other. Hence, development favours differentiation, a new variety normally having better chances to survive (11).

Durkheim extends this principle to all domains of society, including the academic part of it. He also expressed his fears about an exaggerated scientific specialization, which according to him would lead to an anomical state of affairs (12).

Clark does not seem to share this apprehension. When quoting Durkheim, he chooses i.a. some words about the adaptability of the complicated academic organization (13). On the whole, according to Clark, adaptability is a very typical trait of higher education systems, its peak being the US type of university. We may quote a very central thesis of his book:

> The fundamental adaptive mechanism of universities and larger academic systems is the capacity to add and subtract fields of knowledge and related units without disturbing all the others (14).

It is noteworthy that Clark also includes the subtraction of fields in his harmonious picture, his thesis not only being applicable to periods of absolute expansion. In his only example, he tells us that the academic introduction of biochemistry may concern some biologists and chemists but absolutely no sociologists or humanists.

Historically, the thesis is false. In the nineteenth century, it was philosophers and other men of letters who fought against the introduction of e.g. medical physiology at the universities. It was the members of the medical faculty who normally were the defenders of the new discipline (15). Even today, we have controversies of this kind engaging the whole university system. The choice between "soft science" and "hard science", in the epoch of Reaganism and Thatcherism, has a similar importance to the humanist sciences as to technology. Personally, I must confess that I am much more interested in the fate of disciplines concerned with alternative technology or social perspectives on technology than in the quarrels going on at the philosophical department which is may neighbour.

These examples, which easily may be multiplied, tell us that the case of new or threatened disciplines has not only to do with the immediate interest of one's own field. Here, the main point is about the ideals of science and, in the last instance, about the proper use of science in society. When interested in the fate of technology, I am not interested as a specialist but as a man interested in the social role of science. In this case, I share the perspective of the man outside the system of higher education. To him, the struggles among professors are of practically no importance, whereas the effects and uses of science occupy his mind as it affects his own life and environment. And in the long run, this university outsider, who may be a politician, an entrepreneur, a trade-union leader, or a journalist, influences the development and, indirectly, even the specialization of science!

3. On the necessity of an externalist perspective on higher education

Our reasoning has reached the problem of the relationship of higher education to the surrounding society. In his study, Clark analyses the inner life of the higher education system and brings out comparisons of different academic cultures in a very lucid and fruitful way.

Already in the introduction, he maintains that an internalist perspective will be appropriate, the relative constancy of the academic system through shifting historical and social circumstances being the best proof of its relative independence (16). In fact, the only non-academic factors that he takes into account in his study are authorities with a direct influence on higher education (governments, trustees etc.).

All my reasoning till now exposes my conviction that the higher education system must be seen in a much broader perspective. Even the inner differentiation of scientific institutions is fundamentally determined by its role in society.

Of course, I do not deny the relative constancy of universities. The question is whether the university is more constant than other social institutions? You may say that marriage is just as constant over an enormous space of time. The point seems to be that society has a wide range of relatively unaltered forms that can be filled by very different contents (17). I am more interested in the content of higher education than in its forms. Of course, we have to scrutinize the forms very intensely just as Clark does. However, to me the changing content seems to be the real thing.

With content I mean first of all uses of science, or more broadly all uses of the knowledge produced or reproduced inside the higher education system. This is the fundamental social significance of science. Here, we know that the historical changes are enormous and that the national differences are highly substantial. The university is just as a vessel that may be filled by very different liquids, from milk to melted iron.

We must add that the question of what content is to be preferred is highly controversial. Nobody seems to deny that we need some kind of higher education system similar to that which we already have, even the most far-reaching proposals of reform being always partial. Concerning the aims of science and higher knowledge, however, there are a wide range of different opinions, from the idea of research whose sole raison d'etre is to deliver products valuable for the increasing poser of the nation to the claim that the highest aim of higher education is to make us better men. You may object that different brancnes of science are suitable for different tasks, the former being most of all a duty for weapon technology, the latter for ethics or theology. But this is not quite true. Every branch of science is influenced by the main use of scientific knowledge in society. Ethics and theology may serve to make ordinary people better warriors, just as we can imagine a technology serving everything but war.

What I am saying may be misinterpreted to mean that scientific knowledge or research has no intrinsic character. In fact, I finc it very important to distinguish between internal and external perspectives on science. However, what I am talking about here is the social use of science in contrast to the relatively stable form of universities and other institutions of higher education. The university has only for less than 200 years of its long career served as a place were research and hence internal development of scientific knowledge is a fundamental task.

Let us turn back to our main line of reasoning. My point is that the development of higher education must be seen in its interrelationsnip with all the surrounding society. Especially this is important when discussing the problems of scientific specialization and integration.

Now, let us in conclusion try to apply this principle to actual problems in the higher education system and to its desirable changes.

4. And now?

According to Clark, the US university system's superior adaptability to changing circumstances is due to the sharp dividing line between undergraduate and graduate education (18). At the undergraduate level, studies are still quite unspecified, general education being its aim. Not until the graduate level, the professional training starts, and the students turn into true specialists. As a consequence, the mass invasion of new students during the decades after the Second world war has not been as harmful in the US as in most European countries, where every immatriculating student, whatever he studies, has to be treated as a future professional.

Professor Clark's point is very important. In Europe, proposals of introducing some kind of obligatory and preliminary level according to the US model have been presented over and over again, but without success. At the same time, evidently the undergraduate system, or more exactly the appropriate content of the undergraduate studies, presents increasing problems to modern, highly specialized universities in the US. An intense discussion on the spirit of liberal education is going on (19).

To my mind, the problem of general education, or "allgemeine Bildung" - using the more comprehensive German expression, which includes also "general culture" (20), - is one of the most urgent problems in all education today. To some extent, it calls for a real reconstruction. In most respects, our ideal of general education is a pale imitation of an ideal that was really vital half a century ago. We have to start afresh asking what general education may be like in the latest part of the 20th century.

The problem must be much more difficult nowadays than in a situation where departmentalization was not as far-reaching as it is today. I think that we are making a dangerous mistake when presupposing that general knowledge is more elementary and has to pave the way for specified knowledge. At least at the university level, I suspect that the general knowledge in many respects is heavier both to the student and to the teacher, not to talk about the scientists who in one way or another have to create it. It is a grave mistake to see general education exclusively as pedagogic problem. It is also a scientific problem of the first rank - the problem of scientific integration.

Needless to say, comprehensive, non-specialized knowledge is both theoretically and practically of utmost importance. Theoretical development requires cooperation between different fields of knowledge, and practically, the lack of real understanding of biological problems among technicians and vice versa, or the historical innocence among sociologists or politologists and vice versa, constitute a real threat to our civilisation.

Consequently, I do not see general education exclusively or even primarily as a problem of good introductory courses. It would not be sufficient if technicians and engineers had to start their formation by studying social, historical or biological problems. Instead the more far-reaching perspectives have to be integrated in the professional training and at the same time constitute an object of research. In

short, we have to give up the dogma that real research and advanced studies must be specialized.

Professor Clark himself provides an example of how general perspectives play a dominant part inside many scientific specialities. With a play of words, of which Auguste Comte was the inventor (21), he adds that many scientists "are specialists at being generalists" (22). Now, these generalizing scientists are still working inside the traditional specialties, even if they influence neighbouring fields. What is required is also generalists making many specialties their own general specialty.

According to my thesis about the real forces behind scientific integration, this means that the "ideological use of science" has to be strengthened, or more precisely: scientific knowledge and its consequences have to be brought into contact with the great and always controversial problems of life and death, society, history, man and nature. Only in this way, the different fields of science will meet again and those problems which are common to them all be in focus of scientific interest. Or, put in another way, it is the traditionally "philosophical" problems that still have the power to unite different scientific specialities. Modern theory of science since Kuhn has told us that each scientist has, normally latently, his philosophical assumptions. If the latency is made manifest, then what is common among the sciences will be clear and its relationship to thriving ideologies in modern society unveiled. Science has not only to deliver useful (or, as in many cases, devastating) tools to society. It has also to make clear its own position and, of course, to criticize prejudices, even its own.

These very general claims have then to be reformulated according to the traditions of different national and local higher education systems. The circumstances of integration are not the same everywhere. When discussing universities, we are often discussing different ideals of universities - the Humboldt university, the modern élite US university and so on - not realizing that those universities did not even exist either in Berlin 1900 or in Berkeley in 1960. In Sweden, we have from time to time tried to reform our higher education in order to get it closer to some foreign university system, Berlin then or Stanford now. However, our national tradition has its own logic. We may and must be influenced, but we cannot imitate. In the case of integration, we have the tradition of public communication of scientific results in Sweden, a tradition that is definitely weaker in most other countries. In another article (23), I have sketched the picture of the typically Swedish university intellectual as a "civil servant close to the people", and I think that both attributes are very important to understand Swedish tradition as well as Sweden's possible future. The civil servant has internalized the bureaucratic demands: even research, he tends to see as a matter of regulations, rules, and legal abstractions. The man close to the people has some very definite ideas of what knowledge and general education is worth to everybody, just as he tries to meet the demands of egality and to realize the necessity of fighting against prejudices by well-grounded arguments everywhere in society, even in the kindergarten. In our double task to deepen specialist knowledge and to integrate the scattered branches of science also in Sweden, we have to revitalize the man close to the people and to weaken his twin brother, the civil servant.

NOTES

1) Pierre Bourdieu, homo academicus. Paris: Les éditions de minuit, 1984.

2) Op.cit. pp. 55seq. See also p. 22: "le champ universitaire est, comme tout champ, le lieu d'une lutte pour déterminer les conditions et les criteres de l'appartenence et de la hiérarchie légitimes..."

3) In this article, I use the word "scientific" in the broad sense, including e.g. the humanities (as in Swedish "vetenskap" or German "Wissenschaft".

4) Clark seems to do the same; see e.g. p. 36.

5) See e.g. Den vetenskapliga specialiseringen: Begrepp, aktuella problem och tillämpningar. Göteborg: Avdelningen för vetenskapsteori, 1977; and Motsatsernas spel: Friedrich Engels' filosofi och 1800-talets vetenskaper, 2 Vols. Lund: Cavefors, 1977) with an abridged German version: Das Spiel der Gegensätze: Friedrich Engels' Philosophie und die Wissehschaften des 19. Jahrhunderts. Frankfurt/Main: Campus, 1986.

6) The examples of organic chemistry and Darvinian biology, I have worked out in Motsatsernas spel, esp. Vol. I, pp. 153-205.

7) Today, it is necessary to talk about pseudo-professionalization as well, viz. in cases where a certain job is protected by the same type of regulations etc. as a real profession but where in reality neither the examination requested is necessary nor the monopoly of the profession can be upheld.

8) See e.g. J. Ben-David, The Scientist's Role in Society: A Comparative Study. Englewood Cliffs: Prentice Hall, 1971; and esp. J. Ben-David and R. Collins, "Social Factors in the Origin of a New Science: The Case of Psychology", American Sociological Review, Vol. 31, 1966, pp. 451 seq.

9) Clark, op.cit., p. 16.

10) Op.cit., p. 16.

11) E. Durkheim, De la division du travail social (1893), 4th ed. Paris, 1922, p. 262.

12) Op.cit., p.347.

13) Clark, op.cit., p. 187. - Some of the typically Durkheimian these will be found on pp. 216 seq. However, Clark is far from uncritical of Durkheim; see p. 221, where he denies the generality of Durkheim's point that differentation always mean group protection.

14) Clark, op.cit., p. 186.

15) In Sweden at the middle of the 19th century, there was a vehement struggle of this kind. The philosopher Christopher Jacob Boström was the leader of the opponents of the introduction of new, "materialist" disciplines inside the faculty of medicine at the

University of Uppsala. In other countries, you can easily find counterparts.

16) Clark, op.cit., p. 4.

17) I am preparing an article, eventually a booklet, on the usefulness of the concepts of form and content to cope with the problems of constancy and change in intellectual history. To make the point very bluntly, the same form may have a variable content, and vice versa.

18) Clark, op.cit., pp. 49 seq.

19) An important contribution to this discussion is Sheldon Rothblatt's article "The Relationship of Undergraduate to Graduate Education", which I have read in a "final draft, Dec. 1984".

20) When used with some sophistication, the German "Bildung", just as the Swedisn "bildning", has both an objective and a subjective side, "Bildung" being on one hand an objective standard and on the other a goal that a man sets up for himself and his own cultivation.

21) A. Comte, Oeuvres..Vol.1: Cours de philosophie positive, Vol.1, Paris 1839, reprinted Paris 1968, pp. 22 seq.

22) Clark, op.cit., p. 103.

23) "Civil Servants Close to the People", which will be published in a special issue, ed. H. Wasser, of the journal History of European Ideas.

Lennart F. Svensson:

HIGHER EDUCATION AND RESEARCH IN AN EDUCATIONAL PERSPECTIVE

Introduction

The sector of higher education and research has expanded and grown in importance in many countries in recent years. This in itself has made the sector of interest to research and led to an increase in the research into higher education. Higher education and research are not a very clearly delimited sector of society, neither are the terms very well defined. The relation between education and research is a large area of inquiry in itself. The character of higher education and research must be expected to vary between countries and cultures and over time. It is a task for research on higher education to further clarify its nature.

The naming of a sector as higher education seems to imply that it is generally seen as educational in nature. This may mean that it is generally seen and delimited from an educational perspective. Is there such an educational perspective? If so, does it mean that this perspective is of special relevance to the understanding of higher education? The name research does not imply in any apparent way that research is educational in nature. May an educational perspective nevertheless contribute to the understanding of research? These are some of the questions that need to be discussed in the present context.

The discipline of Education is regarded in many different ways. There are different traditions in different countries. Within some traditions Education is not really considered a research discipline but merely a field of reflection upon and development of educational practice in close relation to the actual practice. At some universities Education is a faculty or school including several disciplines or subdisciplines representing different perspectives. The position taken here is that there is an educational perspective fundamental to development of educational knowledge, and that it is important to recognise this perspective irrespective of the disciplinary organisation.

Research on higher eduation and educational research

The common theme of the seminar and the theme of this paper imply that research on higher education does not equal educational research. The assumption made is that many disciplines and perspectives may contribute to research on higher education and that Education may contribute as one of these disciplines/perspectives. Educational research is commonly taken to mean research on educational phenomena. This is tautology. It may then sound contradictory that there is research on education that is not educational. The critical point is if there is a difference between what is emant by "educational phenomena" and "education".

Much research on higher education is apparently carried out within other disciplines. If there is an educational perspective defining educational phenomena, then these other disciplines do not study education in the sense of educational phenomena but in some other sense(s).Should we make a distinction between education and educational phenomena? One answer to this question has been that there is no

special educational perspective and that Education as a discipline, if anything, represents a link between other disciplines and educational practice. Education then represents a more arbitrary, global and commonsensical delimitation of a field and of phenomena. More precise knowledge within the field is developed through the use especially of psychological and sociological perspectives.

The view referred to has perhaps been most clearly expressed concerning the relation between Psychology and Education. Glaser (1976) suggests a linking discipline between Psychology and educational practice which he names Psychology of instructions. A similar way of thinking seems also to exist among researchers who would prefer other perspectives. The view is to some extent also supported by the situation within educational research and at departments of Education where research based on different perspectives are carried out. It should be noted that not all possible perspectives are included, but mainly those of philosophy, history, psychology, and sociology.

If one accepts the view of Education suggested above, the discipline would be concerned with the application of some other disciplinary perspectives to educational practice. The function of the discipline would mainly be to mediate between these other disciplines and the sector of education. An additional function might be to integrate the results achieved within the different perspectives. To be integrative on a firm basis would, however, take a perspective which was superordinate in relation to the other perspectives. This brings us back to the question of whether Education is a true discipline.

There is no contradiction between delimiting a sector named higher education and studying this sector from different perspectives, including the educational one. What is important is to clarify the difference and relationship between education as practice and Education as a discipline. Education as practice may be studied from different perspectives, and phenomena may be seen not only as educational but also as psychological, sociological, economic and so on. In a corresponding way, other sectors of society, such as the judiciary sector or the medical sector, may be studied from a lot of different perspectives. However, we would expect the judicial and medical perspectives to have some special relevance to these sectors. It is suggested here that this is also the case with the educational perspective in relation to the sector of higher education.

The history of the educational perspective

The history of the educational perspective is much older than the discipline of Education, which is of rather recent origin. The formation of the discipline seems to have been combined with losing the educational perspective rather than clarifying and strengthening it. It is therefore important to turn to history to clarify the nature of the educational perspective. Since early times the educational perspective has been one of those perspectives from which man and society has been viewed. The perspective has concerned the passing on of traditions and experience from one generation to the next and from some groups and individuals to others. Within societies and states there has been a concern for how to ensure that the "desirable" moral beliefs, knowledge and skills were developed by groups and whole populations.

As the western societies have developed, the sector of education has grown, especially during the twentieth century. Mass education has

increased and become more differentiated. Historically, there has been a variation in emphasis on beliefs and morals compared to knowledge and skills. In the western countries there has been an increasing emphasis on knowledge and skills and a neglect of beliefs and morals, which does not necessarily mean that beliefs and morals are a less important part of education nowadays. It may be that they are just less obvious and rarely discussed.

In the Jewish Old Testament tradition, education of the young as well as the people at large was considered important. It was the concern of priests and scribes and there were well known masters of teaching as we know from the Bible and other sources. In Ancient Greece the Spartans and the Athenians developed elaborate educational systems. There were also reflections on education, of which especially the thinking of Socrates and Plato is well known. The Jewish-Christian and the Greek traditions have had an important influence on the later development in the western countries.

Throughout the Middle Ages education was mainly the concern of the Catholic church and its priests and monks. Later on, through the development of science and the reformation of the church, education also became the concern of other agents in society. Within the developing universities, the educational perspective was one of the perspectives within theology and philosophy.

The first university professor who devoted his effort to developing something like a discipline of Education was Johann Friedrich Herbart. He succeeded Immanuel Kant as professor of Philosophy at the University of Königsberg in 1809. In Sweden Education became a distinct part of Philosophy quite early through Mattias Fremling, who was professor of theoretical philosophy at the University of Lund from 1803 to his death in 1820. He lectured in Education and Didactics as an examination subject.

Herbart made several contributions to the field of Education. His main work in Education is "Allgemeine Pädagogik" published in 1806. Herbart wanted to base Education as a discipline on two other subdisciplines of philosophy, ethics and psychology. Ethics was seen as the basis for the choice of aims and content of education, and Psychology as the basis for how the education should be carried out as to its forms and methods. At the sime time as Education was to be based on Ethics and Psychology, it should provide its own integration and systematisation, which would make it into a separate discipline (Herbart even developed his own Psychology). It was this integration and systematisation that preoccupied Herbart.

The cleavage between content and form present in Herbart's thinking and the related problem of integration have to a very large extent plagued the discipline of Education ever since. Another serious problem was the heavy dependence on Psychology, although it meant seeing Psychology as a supporting discipline rather than the view of Education as the application of the perspective of Psychology to education. Both these shortcomings, from the present point of view, have characterised the further development of the discipline of Education.

When Education developed as a separate discipline at the universities, it came to depend heavily on some other disciplines rather than to emphasise and develop its own fundamental perspective. The disciplines and perspectives involved were Philosophy, History, Psychology, and Sociology mainly. In Sweden extensive work was first carried out within the history of education. This work is very relevant to the

theme dealt with in this section (cf Landqvist, 1963; Björstrand 1961-66). When empirical educational research was developed, there was a strong influence from the development of empirical psychological research. Later on, there was an increasing interest in sociological research, especially during the seventies in relation to the "new sociology", which was also anthropological and etnographical in orientation.

The present state of affairs in Education as a discipline is that the philosophy of education, history of education, educational psychology and sociology of education are seen as main parts of the discipline. Other perspectives also argued for are the organisational, economic and anthropological perspectives. There is usually no qualification of the meaning of the perspectives that clarifies any special educational meaning in addition to that their application concerns educational practice. The educational perspective has been lost to the extent that there are arguments as to whether Education is a science about the individual (like Psychology) or about society (like Sociology). There seems to be an urgent need to call attention to the educational perspective, which throughout history has meant a concern for the passing on and development of knowledge, skills, beliefs, morals and character.

An analogy to Medicine

To discuss the nature of the relation between the educational perspective and educational practice an analogy to Medicine will be used. What is suggested is that Education has a similar relation to the sector of educational practice, in the following called schooling, as Medicine has to the sector of medical care. The analogy is also used to express what is thought to be the general relation between Education and other disciplines in their relation to schooling. There is always a risk in using analogies, and it is important also to consider the differences as a background for interpreting the similarity.

The most fundamental similarity between Medicine and Education, as far as their relation to their sectors of practice is concerned, is that they deal with a content that is also the aim-related content of the practices. The practices as wholes are considered to be medical and educational because the aim claimed to be the aim of the practices and the content related to the aim are medical or educational. Thus, the similarity is that the two disciplines deal with phenomena that are of a kind corresponding to what is thought to be the aim and the aim-related content of the practices.

The similarity distinguishes the two disciplines in their relation to their sectors of practice from the relation of other disciplines to these same sectors. However, although the sectors may be thought of as medical and educational on the grounds mentioned, the functions and activities are not only medical and educational. It could even be questioned if they are mainly medical and educational. Even the activities of physicians and nurses are not only medical, and perhaps not mainly medical. Aspects and parts of the sectors may vary in nature and may be studied from different perspectives, for instance economic, organisational and psychological perspectives.

Although medical phenomena are only parts of medical care, those phenomena are distinctly different from the phenomena delimited through other perspectives applied to the sector. They represent rather small and hidden parts of the sector. However, these parts and

phenomena are the ones that the aim of the sector is said to concern. It is here suggested that we have a corresponding situation within the sector of schooling, i.e. that it is considered to be educational because its aim is related to educational phenomena. It is also suggested that the educational phenomena within schooling represent rather small and hidden parts of the sector, which are only delimited within an educational perspective.

We mentioned earlier the position that Education equals the application of some other perspectives to the sector of schooling. In our analogy to Medicine, this would correspond to the idea that Medicine was the application of some other perspectives to the sector of medical care. Either it might be the application of social science perspectives, such as Psychology and Economy or the application of natural science perspectives, such as Biology and Chemistry, and there would be no medical perspective. All these perspectives might be applied to the sector, but we should still have no Medicine. In a corresponding way, the application of other perspectives to the sector of schooling could not be said to make up Education.

If the analogy to Medicine and medical care is correct, then Education has an important contribution to make to the sector of schooling. In making this analogy to Medicine, we have emphasised a general similarity as to the nature of the medical and educational perspectives in relation to the medical and educational sectors of practices. It is also important to pay attention to the general differences clarifying the meaning of the similarity. The educational perspective is closer to perspectives focussing on the social and administrative aspects of the sector, which are the more visible and easily focussed on aspects and parts of the sector. The medical perspective, on the other hand, delimits an aspect of the sector which is recognised as quite separate from other aspects of medical care.

The phenomena focussed on within the psychological, sociological, organisational and other social science perspectives include parts that also may form parts of educational phenomena. This is so in the sense that those parts may form conditions related to educational consequences, where the relation between conditions and consequences represents an educational phenomenon. The relation of Education to the social sciences in this respect is more like the relation of Medicine to the natural sciences. However, Biology and Chemistry do not at the same time delimit aspects of social practices, as is the case with the social sciences. Rather, they are disciplines "behind" Medicine. The social sciences may be said to have a double role in relation to schooling. On the one hand, they may be disciplines "behind" Education, on the other hand, they may also be directly concerned with the sector of schooling. These two roles are very different.

There is a further important difference compared to Medicine concerning the relation to other disciplines. Education has a relation to all other disciplines as representing traditions of knowledge that may be related to the content of the educational phenomena. It is as if there had been a great variety of disciplines describing symptoms of different kinds of desirable medical states and functions. Like Medicine, Education concerns not the ideal but the actual, and more the less good than the good qualities. The main concern is with the change of qualities from worse to better. Thus Education has a relation to all other disciplines through the similarity between the specific content of some educational phenomena and the content of the disciplines. This is, in some respects, a more important relation than the

relation to the social sciences as supporting or alternative sciences, focussed on earlier.

Phenomena delimited through the educational perspectiv

We have now characterised the educational perspective by giving a description of its history and by making an analogy to Medicine. This gives a delimitation in terms of the general nature of the perspective and its relation to other perspectives. A more precise and specific delimitation may be given, mainly in the form of describing what characterises the phenomena delimited through the perspective. The description of the history of the perspective showed it to concern the passing on and the developing of morals, beliefs, knowledge and skills.

Here we will specify the nature of educational phenomena to be the development of knowing and valuing. Knowing is used to refer to both knowledge and skill together, without making any distinction. Valuing includes beliefs and morals. Knowing and valuing are meant to refer to two interrelated aspects of a bigger whole. Together, these aspects represent a relation between a subject or an agent and an object. The ing-forms are used to indicate that the phenomena are ongoing activities rather than only some results or products of such activities. Thus educational phenomena are thought always to include knowing and valuing, like medical phenomena always include some medical state or function.

One may be concerned with knowing and valuing in different ways, and the use of "development of" is to express in what way the educational perspective focusses on knowing and valuing. The fundamental concern is with change in and of knowing and valuing. Knowing and valuing are approached and given their meaning within a concern for change. The change is seen as development, in the sense that the concern is for systematisation of qualitative and quantitative aspects of the change. Thus, the concern for the development of knowing and valuing is taken to express and define the educational perspective. The perspective includes conditions for the development of knowing and valuing, which means that many kinds of factors may be included. Thus, the development of knowing and valuing and its conditions are considered to constitute the educational phenomena.

The kind of agent involved in an educational phenomenon may be an individual, a group, an institution, a society, a culture, or perhaps some other kind of agent. It is not the character of the agent that corresponds to the perspective, but the character of the agent's relation to some part of the world in terms of the development of knowing and valuing. It is not the agents as units that are the objects of investigation but their relation to the world. Aspects and parts of the different kinds of units involved may constitute parts of educational phenomena as conditions for development of knowing and valuing.

Knowing and valuing only represent parts of educational phenomena. Educational questions include the conditions for the development of the relations of knowing and valuing. There are many conditions related to the development of knowing and valuing. The conditions may generally be seen as psychological, social, cultural, economic, organisational and so on. The dependence in educational research on some other disciplines has been combined with focussing on certain kinds of conditions.

It is important to realise that it is not the conditions that are focussed on or included that inform us of the perspective, but what they are seen as conditions for, which should also influence the delimitation of and meaning given to the conditions. Thus, for instance, psychological and social factors seen as conditions for the development of knowing and valuing represent parts of educational phenomena. Corresponding factors that are not seen and given meaning as conditions for the development of knowing and valuing are not parts of educational phenomena, and the research is then psychological, sociological and so on, rather than educational.

The relating of conditions to the development of knowing and valuing is usually limited and unclear. The emphasis is on the conditions, and the description of the development of knowing and valuing is very limited. The focussing on the conditions and the development of knowing and valuing creates a situation in which it is suggested that the perspective is educational, while at the same time psychological or sociological perspectives appear to dominate. One reason for the lack of satisfactory descriptions of the development of knowing and valuing is that such descriptions represent the most difficult part of educational research.

External conditions (external to knowing and valuing) are often related only to parts of the bigger whole of the development of knowing and valuing. Often the parts included are later stages of the development having the character of consequences. There is a need of describing the educational phenomena more fully. In particular, it would be fruitful to relate external conditions to conditions of knowing and valuing and not directly to the consequences. This would mean that one should be careful to consider conditions of knowing and valuing and their relation to knowing and valuing as consequences. Thus, the educational perspective delimits educational phenomena in the form of a great variety of external factors related to internal conditions of knowing and valuing in themselves related to change in knowing and valuing.

Contributions to the understanding of higher education

Having suggested what the nature of the educational perspective is, it remains to suggest what contribution this perspective makes to the understanding of higher education and research. However, what is meant by higher education is also problematic (cf Barnett, 1985). Higher education is perhaps first of all thought of as a sector in society mainly on the basis of its organisation. Since the identification of higher education is mainly based on its organisation, organisation also tends to become the focus of research. From the present point of view this represents a problem and a risk. The tendency is strengthened by the expansion and organisational development of the sector against the background of a general increase in bureaucracy and a dominance of the bureaucratic-administrative perspective in society and also in educational practice.

Against the background mentioned above, one would expect the expansion of research on higher education to concern the organisation of higher education. This also seems to be the case. In the research programme of the National Swedish Board of Universities and Colleges there has been a change in emphasis from educational phenomena to organisation. In line with this development, a book by Burton Clark (Clark, 1985) on "The higher education system. Academic organisation in cross-national

perspective." was given as a common point of reference to the authors contributing to the present seminar.

The emphasis on organisation is understandable, but at the same time somewhat paradoxical. In no other part of society or even of educational practice is there such a concern for the development of knowing and valuing, mainly in the form of a concern for the development of knowledge and skills. The educational phenomena involved are very complex and little understood, and there are many and fundamental problems in dealing with these phenomena in practice. The educational perspective is still very much neglected, both in practice and in research, in favour of administrative and organisational perspectives.

The book by Clark represents a work within the sociology of education based on an organisational perspective. The theme dealt with concerns general principles of organisation of higher education in different countries. This certainly is a legitimate theme, and the presentation is very illuminating. The book will not be commented on or criticised on its own premises. What is considered here is its relation to the educational perspective. What is focussed on is what may be contributed by work within an educational perspective, which is missing in the work by Clark, because it is carried out within an organisational perspective. In relation to Clark's work, the educational perspective might contribute directly in two ways, by giving a firmer basis for delimiting the sector of higher education and by giving an alternative or complementary description and comparison of higher education in different countries.

The delimitation of the sector of higher education

It is suggested here that educational criteria are more fundamental to the delimitation of higher education than organisational criteria. However, the organisation is supposed to be based on the educational nature of the sector. As the educational character of the sector is more difficult to establish than the organisational character, it is convenient to use organisational criteria. A further use of the educational perspective may give a more precise and fruitful delimitation of the sector. There are several differentiations to be made in the delimitation of higher education. Perhaps the most difficult to make on an organisational basis is to differentiate education from research. We will return to this problem below, when discussing the contribution of the educational perspective to the understanding of research. Another differentiation to be made is in relation to other sectors of society (not education). A third differentiation is in relation to other forms of education.

The differentiation from non-educational sectors may be based mainly on aims and intentions, activities and content and/or results and functions. The grouping of organisational units in sectors is based on the parts of the aims, content and/or results considered to be the dominant or important ones. This means that not all parts of educational organisational units are mainly educational in nature. What is said to be higher education in one country may differ from what is said to be higher education in another, depending on the extent to which non-educational units are included. Also, differences concerning the educational nature of higher education would be an important basis for understanding the meaning of organisational differences. The clarification of educational differences between sectors requires a characterisation and comparison of parts of the sectors. Such clarification and characterisation would also provide a basis for a more pre-

cise delimitation of the sectors by giving better criteria and knowledge for including/excluding borderline parts of the sectors and establishing its limits.

A further delimitation to make is in relation to other forms of education. This delimitation is perhaps fairly obvious as far as organisation is concerned. It may be difficult to make such a distinction on the basis of the nature of the educational phenomena, i.e. in terms of how the development of knowing and valuing differs in higher education. However, such a characterisation seems important if one wants to compare higher education in different countries, and even if one wants to relate higher education to other forms of education.

Characterisastion of the sector of higher education

What has been suggested above is that educational organisations and parts of such organisations may be delimited on educational grounds. This might be a contribution to any kind of research on higher education. On the basis of the delimitation one might, for instance, focus on the organisation, as Clark has done. To make the delimitation within an educational perspective, of course, is especially relevant if one wants to focus on educational phenomena in characterising higher education. As mentioned above, to compare sectors of higher education in different countries as to their educational nature would be a complement to the study presented by Clark.

The introductory section in Clark's book discusses knowledge as the material dealt with in higher education. The book continues by analysing principles of organisation and differences between countries in this respect. The alternative in an educational perspective would be to continue the focus on knowledge and analyse how what is said to be the material dealt with, i.e. what is the content of higher education. In giving a description of the sector of higher education, two interrelated aspects would be central. What is the specific content of the sector in terms of educational phenomena, and how is this content organised into bigger wholes making up the whole sector? Comparisons between countries would then concern differences in content and organisation of content.

It is suggested that an educational perspective means a focus on the content of higher education. Content then consists of cases of development of knowing and valuing and external factors related to this development, according to our earlier delimitation of the educational perspective and educational phenomena. However, the field of educational phenomena within higher education is almost unlimited, and restrictions have to be put on any description of the sector. The sector is commonly described in terms of some of the external factors related to the development of knowing and valuing, especially factors regulated through the organisation of higher education. This corresponds to describing what is usually called the curriculum. The meaning of curriculum has developed over time. Curriculum research first focussed on syllabuses but now concerns more or less all the regulated conditions for the development of knowing and valuing (cf Tyler, 1950; Beauchamp, 1975 and Apple, 1979).

Factors focussed on are the goals set, the teaching-learning material included, the way of organising material and activities, the time devoted to different activities and the control of results. On the basis of assumptions about the role of these external factors, one might describe and compare higher eduation educationally. This is

convenient for several reasons. It allows the whole sector to be covered. It is based on observable and relatively stable characteristics, and it concerns factors that may be directly influenced by political-administrative decisions.

Analyses of curricula represent a contribution within an educational perspective, if it is assumed that curricula tell something about the development of knowing and valuing. Curricula of higher education may then be compared to clarify differences between countries in "traditions" of development of knowing and valuing, as well as changes over time within countries. Comparisons between countries have been made throughout the history of education (cf Noah and Eckstein, 1969). Also, comparisons with other forms of education might be carried out on the same basis. Such comparisons are important not only in characterising higher education but also in illuminating the relation of higher education to the education preceding and following it.

However, the problem is that descriptions of some external factors say little about the nature of the educational phenomena and the assumptions made may be wrong. To solve this problem, the relation between the external factors, the curriculum and the development of knowing and valuing has to be investigated. However, the main reason for studying this relation is not to be able to characterise the sector of higher education but to find out about the nature of the educational phenomena. The understanding of the nature of educational phenomena, of course, is the main contribution within an educational perspective. Before tuning to this, we will comment on its contribution to the clarification of another general issue. It is the question of whether higher education is really educational.

The function of higher education

The aim and function of higher education is mostly thought of as educational. Within the sociological perspective, however, another aim and function of educational practice is suggested (cf Young, 1971 and Giroux, 1983). The aim and function is said to be the controlling, selecting and sorting of individuals and groups in society to ensure the reproduction and stability of the society. If what is suggested is true, it indicates that educational practice is something more than being educational, that what is educational just represents one aspect of the practice. This is a position accepted from the start of this paper.

If one wants to argue not only that higher education means sorting and reproduction, but that this is its genuine or true nature, the situation is different. The position taken may be that higher education has no educational function at all, or that its main function is not revealed in an educational but in a sociological perspective. To justify this position, it is not enough to show that higher education has the function suggested within a sociological perspective. One has to investigate what educational functions it has or has not, and this must be done on the basis of the educational perspective.

If one finds that higher education has an educational function, which is perhaps not the one thought of or claimed, then the question of what is its main or fundamental function may not be answered within one of the perspectives, but only by relating the phenomena of higher education as seen within the two perspectives and by an understanding of the relation between its educational and sociological functions. One will then find that the primary aim and function of higher educa-

tion is educational, and that the sociological function is sustained through the educational function. Also, the educational function far extends the sustaining of the sociological function mentioned. Of course, it is possible to take the view, that it is the secondary sociological function that is the important one.

Attention has now been drawn to the contribution of the educational perspective to the delimitation, the characterisation and the establishment of the function of higher education. These contributions concern the differentiation of the sector of higher education as educational and as educationally "higher". The differentiation and characterisation of the sector is dependent on knowledge about the educational phenomena of the sector. The more extensive and deep this knowledge, the better the differentiation and characterisation. The main aim of developing knowledge about the educational phenomena, however, is that it provides a basis for changing and improving higher education.

Development in knowing and valuing

Educational phenomena represent parts of higher education. The educational aspect concerns the development of knowing and valuing. This aspect could be focussed on in itself, or the focus could be on the relation of other aspects to this aspect focussed on. In both cases, it is the aspect of knowing and valuing that is the one started from and illuminated. In this section we are concentrating on the aspect itself and change within the aspect, as shown by the use of the word in in the heading. In the next section we shall deal with the relation of other aspects to this development.

Knowing and valuing is not only of something but also of someone. There is always a subject or an agent. The agents in higher education may be of different kinds. They may be individuals, groups or institutions. The agents may have different roles and functions. The individuals, for instance, may be students, teachers or administrators. The development of knowing and valuing by teachers and administrators or by groups of these or by institutions is expected to contribute to improved conditions for the development of knowing and valuing by students.

Thus, higher education includes educational phenomena, the development of knowing and valuing among teachers and administrators, that do not represent aims but rather conditions for achieving the aims. A study of the development of knowing and valuing among teachers and administrators would be research within an educational perspective, although not directly related to the aim of the practice. In a similar way, one could investigate any practice from an educational perspective, even when the aim of the practice is not educational. The educational aim of higher education is mainly tied to the students, for which reason educational phenomena that involve the students as the agents have a special significance.

Development of knowing and valuing may be described in many different ways. The development may be prescribed in a curriculum, and the prescription may also be taken as a description under the assumption that it is followed. Thus, one may assume that through the curriculum, through course descriptions and descriptions of examination tasks one knows what knowing and valuing are developed. This is a very strong assumption, and the possibility of it being true should be expected to be very small. Still, it is very common to make this assumption.

If one questions that the actual development of knowing and valuing corresponds to the prescribed development, there is a need for describing the actual development, which raises the question of how this should be done. Empirical descriptions of education are often similar to the prescriptions, which means that they are not really descriptions of knowing and valuing nor of the development of knowing and valuing, but merely descriptions of what areas of content that have been dealt with. One may observe what content areas are dealt with, and to what extent, and compare this with the prescriptions. This gives valuable information about what development of knowing and valuing may have taken place due to participating in the course in question. .pa However, it gives no description of the development of knowing and valuing actually taking place.

The aims of higher education are prescribed as knowing and valuing, and the achievement of the aims is often controlled by use of examinations. The description of knowing represented by the examination results is very restricted. It concerns the manifestation of the right knowing in relation to standards. The control of the meeting of certain standards of performance at a given point in time, however, says very little about both the character of the knowing and the development of the knowing.

Very different descriptions of the development of knowing and valuing have been developed in two research projects financed by the National Swedish Board of Universities and Colleges. Both projects concern groups studying to become physicians, technicians, psychologists or economists. In one project, what students think about education, professions and society is described (Franke-Wikberg and Zetterström, 1984). In the other project, what students think about central themes within the four areas of education is described (Dahlgren, 1984; Dahlgren and Pramling, 1985). The investigations are longitudinal, and there is an emphasis on the difference in thinking between the beginning and the end of the students.

The regular prescriptions and descriptions of the materials dealt with and the examination performances are usually descriptions in "administrative categories", pointing out areas of content and performance. The descriptions appear to be exhaustive, since both what is dealt with and what is achieved seems to be covered. However, a thorough description of educational phenomena requires a description of the character of the dealing with the material and the manifested examination performance in terms of development of knowing and valuing.

Descriptions of what material is dealt with and control of standards through examinations, used to clarify the development of knowing and valuing, of course, represent an educational perspective. The problem is that these indications of the educational phenomena are insufficient to reach a more valid description and understanding. The reasons for this are of two kinds. First the activity of the student is described in too limited a way, both with regard to the material dealt with and the standards met. Secondly, change in knowing and valuing is usually not described at all. The two kinds of shortcomings are interrelated, and together they mean several missing links in the description of development of knowing and valuing.

A more full realisation of the educational perspective would mean a description of the dealing with material and the carrying out of examination tasks both in terms of knowing and valuing and in terms of changes in knowing and valuing. The knowledge and understanding that

such descriptions may give is very much lacking within higher education. It is a very fundamental kind of knowledge in relation to the practice, corresponding to knowledge about diseases within the sector of medical care (cf above). Thus, descriptions of the development in knowing and valuing are taken to be the most fundamental contribution within the educational perspective to an understanding of educational practice.

Conditions for developing knowing and valuing

In the preceding section, the development in knowing and valuing was commented on as the field for fundamental educational research. In this section, the interaction with external conditions in developing knowing and valuing will be emphasised as a central field for educational research. The development in itself may be described, as emphasised in the preceding section, but it may also be described as dependent upon external factors (external to the knowing and valuing). While the development in knowing and valuing is fundamental, describing its relation to external factors gives a more full description of the educational phenomena.

Including the external factors is important not only as an addition to describing development in knowing and valuing, but also because inclusion of them may deepen the understanding of this development. The understanding of the role of external factors also provides a basis for attempts to influence the development, which is very central to the practice. The concern for influencing development, involves the risk that the external factors in themselves will be focussed on, rather than their relation to the development of knowing and valuing. As a consequence, descriptions of the development of knowing and valuing will be limited or shallow or not taken as the starting point. The description of the phenomena may be made mainly in terms of a development of the agents' interaction with external factors, which would mean that the educational perspective is lost.

The external factors are of many kinds. One kind of factor is that represented by the activity of the agent developing the knowing and valuing. The study activity of the students in a sense represents the most immediate context of the development of knowing and valuing. Student learning has developed as a field of research during the seventies and eighties. Even in investigating study activity, there is a risk of focussing on factors external to the development of knowing and valuing without dealing with them as conditions for this development. However, quite a large part of the investigations have concerned aspects and parts of study activity as they are related to the development of knowing and valuing (cf Choppin, 1973; Beach, 1974; Entwistle et al, 1974; Marton et al, 1984).

One of the first projects within the Programme for research into higher education in Sweden concerned studies in English as a foreign language at university. Descriptions of the relation between the students' initial performance, their study activity and their final results were developed (Svensson, 1970; Marton, 1972). In a later project, descriptions of study activity and study success were developed for two groups of students, one group studying Education and another a combination of social science subjects (Svensson, 1976). These descriptions were combined with an analysis of students' reading and understanding of texts and their thinking about phenomena dealt with in the studies (Dahlgren, 1975; Säljö, 1975; Svensson, 1976). In still later projects, further descriptions of students' understanding

and thinking have been developed, especially within the fields of Economics (Dahlgren, 1978 and 1986) and Mechanics (Johansson, Marton and Svensson, 1985).

The activities of teachers represent another kind of factor. These factors are crucial to the educational practice for several reasons. They are characteristics of relations between persons. Relations between persons hold special possibilities. The teacher is also an agent in relation to other external conditions for the development of knowing and valuing, including the activity of the student. The teacher is expected to influence the activity of the student and to mediate between the material and the organisation of it and the development of knowing and valuing by the student. He is also expected to bridge social and cultural gaps between the prerequisites of the student and the demands of the educational situation. There has been some research on the thinking and activities of teachers in higher education, but in most cases without any clear relation to the activity and/or development of the knowing of the students (Trent and Cohen, 1973; Dunkin with Barnes, 1986).

Many other factors might be mentioned, such as material, course design, social climate, pattern of social interaction, time for teaching-studying, physical facilities, cultural background, ideologies and so on. These and other factors may all be related to the development of knowing and valuing. Because there are so many and because they are of varying importance in different cases, it is very important to stick to the focus on the development of knowing and valuing and not to end up describing these factors per se. To describe these factors per se belongs to other perspectives and not to the educational perspective.

The importance of teachers' activities is not due to the fact that they are more important than other situational factors in understanding the nature of individual cases of development of knowing and valuing. However, they are often crucial to the understanding of why these cases came about. Most of the other factors in the situation may be expected to be more fixed, while teacher activity may be expected to vary. The teacher is expected to specify the meaning of the situation to the student. The teacher's activity is strategic in giving meaning to other factors in the situation. It is the role of the teacher to create subjective situations for the student that will bring about development of knowing and valuing. Because of this role of teachers, an educational perspective and knowledge about student activity and the development of knowing and valuing is an essential part of the teacher's equipment.

Research into the activities of teachers and their interactions with students has a rather long tradition. However, there has been a strong tendency to focus on the activities and interactions per se or in relation to other external factors such as different aspects of curricula, without relating the activity and interaction to the development of knowing and valuing among the students. Such studies may be intended to be educational, i.e. the aim may be to relate the external factors to the development of knowing and valuing, in a general way, indirectly or perhaps later on. Here we are bordering on studies within other perspectives, such as the social-psychological or organisational ones.

The description of all kinds of factors as conditions of the development of knowing and valuing, to find out the importance of these

factors to the development, is here said to represent an educational perspective. There is a great need of such descriptions. They are cotributions to be made within the educational perspective.

Knowledge about educational phenomena outside higher education

One kind of knowledge that may be important to higher education is experimental knowledge developed in situations which are not regular situations within higher education, i.e. artificial situations. For ethical and practical reasons, such situations have to be rather restricted in scope and mainly concern knowing rather than valuing. When it comes to knowledge about the development of knowing, experimental research represents a very little used possibility. More specific knowledge of conditions for the development of the kind of knowing aimed at in higher education would be an important contribution to the understanding and improvement of higher education.

Another area of knowledge concerns educational phenomena within education outside higher education. Of special interest is schooling that directly precedes higher education. As the development of knowing and valuing is always in some sense a further development of what is already there, the relation to what is already there is crucial. In higher education assumptions are made about what is already achieved in earlier schooling. The assumptions may be explicitly stated or implicit in the setting of goals and the design of courses and materials. For the student there is often a wide gap between his actual prerequisites and what is assumed or expected to be the starting point.

The knowing and valuing dealt with in higher education are often more complex and extensive compared to what is the case in earlier schooling. Also, the students are expected to be more independent and responsible. The background, age and role of the students influence the role of teachers and the way of organising the activities. Thus, a difference between educational phenomena within earlier schooling and higher education should be expected. The difference concerns both the development in knowing and valuing, the relation between prerequisites and achievements, and the relation of the development to external conditions.

A third area of knowledge concerns educational phenomena outside education (schooling). Of special interest to higher education is knowledge about the educational phenomena within professional life prepared for by higher education, i.e. the further development of knowing and valuing. Such knowledge would also provide a further basis for the delimitation of higher education within an educational perspective, discussed earlier. This is a large area for research, and it can be widened still further to encompass the total development of knowing and valuing in society, which also represents the societal and cultural context in which the development of knowing and valuing by individual agents in society must be understood. The contribution of the educational perspective to the understanding of higher education also concerns the understanding of the development of knowing and valuing within higher education in relation to the development of knowing and valuing in other parts of society and in society at large.

The educational perspective and research

In this paper we have discussed the contribution of the educational perspective to the understanding of higher education without paying any attention to its relation to research. Looking at the external factors related to the students' development of knowing and valuing, the relation of those factors to research is very important. Thus, the indirect relation of research to the development of knowing and valuing is a concern within the educational perspective. What about the research in itself? May the educational perspective contribute to our understanding of research?

Here we want to remind of the close relation between research and teaching throughout the history of the universitites (cf Schulman, 1986). Historically, the educational function has in a sense been superordinate and the criterion for the judgement of research. It was when knowledge was developed, so that it could be taught, that it was considered to meet the standards of knowledge required at the university. The scholar was a scholar by being a good teacher of old and new insights. This is reflected in the fact that the highest degrees awarded at any university are those of "master" and "doctor", which both mean teacher. The same emphasis was expressed through the importance given to disputations. Later on, research became less closely associated with teaching and education. Rather than being a natural sequence of research and teaching, there has developed a conflict, research or teaching, where research is mostly seen as the most profound activity.

The educational perspective has been defined as focussing on the development of knowing and valuing. This is a definition which is very close to the essential meaning of research. If research should be distinguished from education and vice versa, we have to add further qualifications to the definition of the phenomena. Even so, the focus on development of knowledge is common, which makes the phenomena similar in a fundamental sense. In research the aim and, hopefully, the results concern collectively new knowledge, which according to certain criteria is better than the old knowledge. There is very much an emphasis on konwledge as a product. In education the aim is for the spreading of already existing knowing and valuing. The emphasis is on knowing as an activity and in relation to valuing. However, what is learned is always new to the learner, which brings education close to research as to the nature of the activity.

Some research phenomena and educational phenomena within higher education may be difficult to separate, especially within doctoral programmes. It may happen at a certain stage in his work that the student develops collectively new knowledge and teaches the supervisor rather than the other way around. Irrespective of who is actually teaching, the teaching may be intimately related to the development of new knowledge. (A very interesting project concerning suvervision in doctoral programmes is being carried out at present within the Programme of research into higher education financed by the National Swedish Board of Universities and Colleges; cf Lybeck and Asplund-Carlsson, 1986.) A researcher's teaching may more generally be a part of his developing new knowledge. It may be a way of explicating his present understanding of some phenomena. However, research and educational phenomena may be further differentiated through the relation to external factors. It is through a more full realisation of the educational perspective that the phenomena may be separated.

As education and research are fundamentally similar in nature, the educational perspective will be able to contribute to the understanding of research through the description of the educational phenomena involved in research. The educational perspective on research would focus on the development of knowing and valuing generally, the unsuccessful and not accepted as well as the successful and accepted. The focus would be not on the products per se in relation to criteria, but on the development of knowing and valuing. Of special interest would be the mediation and development of traditions of knowing and valuing, as well as how researchers teach and learn from each other what they have developed through their own research.

Concluding remarks

It has been stated in this paper that the educational perspective focusses on the development of knowing and valuing, that this is also thought to be the aim of higher education and that this is why higher education is thought of as educational and named education. Further, it has been said that the educational phenomena may be understood, in a more complete sense, as development seen in relation to conditions external to the knowing and valuing. The external factors may be of many kinds and in themselves seen as psychological, sociological, organisational, cultural and so on. These factors form parts of educational phenomena only to the extent that they are understood as conditions for the development of knowing and valuing. When it concerns these factors, the contribution of the educational perspective is the understanding of them as conditions of the development in knowing and valuing.

Thus, the contribution of the educational perspective to the understanding of higher education and research concerns the development of knowing and valuing and the conditions of this development. This is an understanding that is fundamental to higher education and research. In relation to non-educational sectors of society, as well, it is an important perspective, because development of knowing and valuing may be or might be an aspect or part of the sector, although the sector and the activities are mainly thought of as a different kind, for instance as work or as a hobby.

The educational perspective delimits a great variety of phenomena within higher education. The content and the conditions of development of knowing and valuing represent an unlimited field of research. The educational practice within higher education is mainly based on common-sense thinking about the nature of the educational phenomena. The assumptions made about the phenomena are to a great extent in conflict with what may be expected to be the nature of the phenomena on the basis of the limited research carried out so far. Nevertheless, there is a risk that the need for knowledge in these respects will not be recognised. The most profound lack of knowledge is perhaps the hardest to become aware of.

The development of knowing and valuing is the heart of the matter. However, it may be seen as unproblematic. Research into higher education may easily be dominated by research into organisational and administrative aspects (even educational research). The first thing needed, from the present point of view, is to obtain recognition of the fact that the field of educational phenomena is an unexplored one. The fact tends to be ignored even within Education as a discipline.

This is why writing this paper has felt both important and challenging. It is hoped that the situation described will also present a challenge to the readers.

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Katrin Fridjonsdottir:

SOCIOLOGY AND ORGANISED KNOWLEDGE: CHANGING PERSPECTIVES IN THE SOCIOLOGY OF SCIENCE

Introduction

An invitation to this conference states that "Scientific knowledge of the content, forms and effects of higher education and research has grown considerably over the past 10-15 years, both in scope and in abundance of detail, but at the same time the wholeness and relationships within this problem field would seem to have grown more ambigious and complicated". This can fairly be termed one view of this field of studies. As regards the contributions made by the various social sciences and humanities toward this expansion, we also find that it is, not least, the sociologists who have helped to provide this wide, variegated and perhaps also confusing picture. This makes it both a challenging and a somewhat complicated task to appraise and exemplify the ability of the disciplinary perspective of sociology to elucidate and explain fundamental questions in this field of knowledge, especially as regards a holistic approach which is also asked for in the invitation.

Let me add that I believe strongly in the need for a more comprehensive social science approach in this field of study, at the moment rather much in flux as to its theoretical development. But such a comprehensive approach might perhaps not be the outcome of the efforts of one social science discipline only, but rather result from joint efforts by researchers of different social sciences. Such efforts will, however, certainly have to include the sociological perspective.

In this paper I will take you through a journey that goes approximately from classical sociology to the modern contributions in the field of social studies of science in the 1980s. This historical account is made not only in order to illustrate the scope of the different agendas and themes of sociologists as how to explain science and its relation to society, but also in order to illustrate the development and fate of the different programmes. Some of the questions raised at an early stage of disciplinary development now seem somewhat inadequate, at least in today's research, other agendas seem to have been "buried" only for a while and then return in the programmes of modern social studies of science. The development itself might therefore also tell us something about the nature of social science development, and give some suggestions as to the role of different (disciplinary and social) contexts in this development.

The awareness of the role of context in scientific development, is here also regarded as one main achievement of post-1970 sociology of science and modern social studies of science. There are, however, some problems in current studies as how to relate the different contexts, and how to translate the contextual conditions of science into a more comprehensive theory of the development of science in its relation to societal development. There are lacunae, both empirical and theoretical, waiting for efforts by imaginative sociologists - as well as other social scientists. I will discuss some of these lacunae in the latter part of the paper.

In this paper the focus is thus mainly on one aspect of sociology's preoccupation with organised knowledge: science and scientific research. Studies of higher education, as well as perspectives used in that area of study, are not explicitly dealt with here. As regards modern sociological work in higher education studies, aimed at a comprehensive understanding of university organisation, a recent book by Burton R. Clark was in focus at the conference that led to this volume. My discussion of Clark's book developed into a separate Appendix of this paper.

Sociology and organised knowledge: the early disciplinary heritage(1)

Sociology, a discipline professing to have the culture of modern society as its particular field of study, has always had a special predilection for studying the organisation of knowledge and its implications in order to understand society, but perhaps also in order to understand the role of sociology itself as a science. There are perhaps few subjects that for the sociologist lend themselves so well to combining scientific analysis with disciplinary self-reflection.

Alongside the interests of sociologists in the creation, conditions and transmission of organised knowledge in modern society, there is also the recurrent problem of the sociological nature of organised knowledge itself, raised from time to time, in order to understand our (scientific) view of reality, and the role of science in creating that view. Both kinds of approach are rooted in the disciplinary heritage of sociology itself, a discipline more or less born out of attempts both to scientifically explain the development of social life and to find scientific means to organise social life in modern society. Of course, this can be said to hold good for all social sciences, marked as they are from their very onset by practical interest in political and economic affairs, their aim including the provision of "objective scientific" proofs of the necessity for changes which could perhaps not be supported by "practical" arguments. Especially for the latecomer, sociology, there seemed to exist (and still does) some confusion about the meaning of "scientific laws" when applied to human action, and some uncertainty about the relation and distinction between statements of fact and values.

The beginnings of sociology were dominated, on the one hand, by attempts to describe and analyse social development and, on the other, by efforts to gain ground as an independent science on a level with other social and behavioural sciences. Sociology took its first steps at a time of rapid scientific progress: this was a time when modern natural science was being enlisted more and more in the service of society. While natural sciences and their impact on social development formed one part of the societal activity which the sociologists were trying to describe, natural sciences also presented a successful model for social science to imitate. But it was no less important for sociology to establish an identity distinct from that of the sister-disciplines in the other social and behavioural sciences, disciplines which also professed to explain the principles of human social life in modern society. Moreover, a dual task of the above mentioned kind naturally called for an understanding of the scientific essence of one's own activities, among the pioneers of sociology. The early development of sociology was no concerted action and there were few ties between the pioneers, who tended rather to explore their own trails and in this seldom even referred to each other's work. What united the pioneers of sociology was that all felt the need for a

theory of science (especially a theory of their own science) and they shared an interest in the role of knowledge in societal development.

Concerning the role and nature of knowledge, and, as an early heritage for later sociology of science and higher education, we may mention as examples the works of two of the pioneers: Emile Durkheim, and Max Weber. Given the above mentioned perspectives - the task of sociology as a social science, and the role of knowledge in society - both Durkheim and Weber have bequethed us reflections on the role and nature of organised knowledge in modern societal development. The academic outsider, Karl Marx, should also be mentioned, as some of his writings are important as a heritage to be administrated by later sociologists in the fields of studies of science, technology and higher education. Marx, Durkheim and Weber not only asked important questions and offered fruitful, if varying, answers about the social origins and effects of the European industrial and democratic revolutions. They were also concerned with identifying scientific laws of social organisation and development in general, and with pinning down some of the connections between the development of knowledge and its use in the service of social development. As to how they defined the nature of (social) scientific knowledge they differed and the different views taken by these writers on the conceivable and desirable relationship between the development of knowledge and its societal utilisation must of course be considered in terms of both their different epistemological convictions, as well as of the different contexts in which they were working.

For all of them the historical setting is that of 19th century Europe. The attitude adopted by the (once) Hegelian disciple, Karl Marx, to knowledge and its societal use was that of the revolutionary intellectual and the critically involved scientist. To Marx, the scientific programme was a negation of the capitalist social order which he described. This required both a critique of contemporary social scientists (whom Marx claimed were mainly "ideologists" working for the preservation of the established order) and the definition of scientifically tenable methods for understanding social development. In addition to being scientific, knowledge of the laws of society must, according to Marx, be within the reach of all human beings (and above all the working class) in order to be used in the transformation of society.

As to the direction in which society should be transformed, Marx had firm convictions. As to the analysis of knowledge and the role of knowledge in the transformation, Marx draws heavily on the teaching of Hegel. But while Hegel was convinced that knowledge (Bewusstsein, Geist, Vernunft, in Hegel's terminology) transformed the society, Marx strongly emphasised the importance of the material process of production in the process of transformation and in this connection viewed "material production" and "cognitive production" in dialectic interplay: human history unfolds within a true "totality", of a piece with nature and social life, and in transforming process of interaction. Knowledge comes as a result of this human interaction with the environment in its broad meaning, and knowledge in turn is applied in further interaction.

Marx puts emphasis on the role of material production, and his programme also includes the aspect of power, especially class-power, as an important factor in societal development. More influential in sociology than Marx' general theory of knowledge, has been his contribution to a theory of intellectual production along the line of his general social theory. The basic assumption here is that human con-

sciousness rests on certain material conditions without which it would not exist. (This argument was, of course, of importance for Marx already when attacking the idealism of Hegel.) The argument, however, goes beyond a mere abstract claim that the material base determines the set of ideas in society: there is an intervening set of processes that take account of multiple social classes and their conflicts. Social classes on the upgrade tend to favour progressive theories of societal development, the assumption being that such classes are favoured by the search for new truths. When those classes, however, have taken power in society, they are mostly interested in cementing their class power. Hence, they favour theories that have that function, irrespective of the truth values of the theories. (Marx and Engels even held that irrationalism and mysticism could have a social function, since such tendencies bared insights in true social reality as well as the aspirations of new social classes.) Thus, the reason why the ideologies of the ruling classes, reflecting their own interests, dominate the spheres of ideas in society is because the ruling classes have the capacity to control the material means by which ideas are produced. Intellectuals, including social scientists, although, in principle, "free" to formulate whatever ideas and theories they can conceive, nevertheless tend to create ideologies (and scientific theories) favoring the interests of the class that feeds them.

Marx, and his companion Engels, never developed the theory of the means of intellectual production systematically. But the general conception proved to be quite fruitful in later sociology when looking at various forms of intellectual creation. Mannheim's sociology of knowledge pays debts to Marx in this regard, as well as various studies concerning the changing forms of art and literature in different historical periods. This line of thought is also one starting point for the sociology of science, when in the 1930s J.D. Bernal, J. Needham, and others, attempted to show how science and technology develops within certain historical, societal conditions. Although the sociology of science was not going to proceed along these lines (due, above all, to the challenge to this view brought in by R. Merton, claiming the importance of the inner normative organisation of science for scientific development), Marx' ideas was to become one of the points of departure of modern "externalist" sociology of science and technology as well as later Marxist-inspired studies on the role of higher education.

Marx, who was not himself an academician, did not pay much attention to the "identity-problems" of the various social science disciplines, nor did he pay much attention to the role of the universities. These features were however important problems for Durkheim to deal with and analyse. A few decades after Marx, Durkheim in France took up the mantle of Comte. Durkheim's programme was rather influential in inspiring his successors in sociology in general. Durkheim's approach to his own science was also influenced by a conviction of the responsibility of the social sciences as to the social development of emergent industrial society. This conviction of his - science as a cohesive force in a peaceful and consensual societal development - permeates most of Durkheim's writings. It is, however, his more puzzling study of religion as a cohesive force which seems to have been a more direct source of inspiration to subsequent sociological studies of knowledge and science. (2)

Durkheim's study of religious behaviour is above all concerned with the societal foundations of human convictions and rituals and their symbolic meaning. As such it has inspired anthropological studies of systems of belief in different cultures (Levi-Strauss), and investiga-

tions of what passes as rational behaviour in various subcultures of modern society.(3) Also, Durkheim has been a source of inspiration to modern developments in social studies of science (especially in the "Strong Program"), where Durkheim's view of the contextual dependence of rationality is expanded to apply to scientific rationality as well.(4) The heritage of Durkheim is, however, far from "unproblematic" to apply in modern, disciplinary studies. The Durkheimian concept of collective forms of understanding refers to the ways of thinking and perceiving as well as to what is thought and conceptualised. When based on this "ambiguity", a Durkheimian-inspired sociology of scientific knowledge draws hardly any distinction between a theory of knowledge and a sociology of knowledge, or, between what constitutes valid knowledge and what is taken for valid knowledge in a given social context. It is perhaps symptomatic of the revaluation occuring in sociology of science during the 1970s that Durkheim's ideas were called into service to assert the relativistic aspect of scientific knowledge development.

The responsibility of science and (social) scientists was also an important theme to Max Weber, though in somewhat different terms than to Durkheim. Weber saw in bureaucratisation the fundamental tendency of modern civlisation. Bureaucratisation, in turn, was the manifestation of a more profound and general historical process, that of rationalisation. Furthermore, it was the link between capitalism and the development of the exact sciences which really enabled bureaucracy to grow. Weber in this respect viewed the development of modern science as part of a necessary - though not altogether desirable - process of modernisation of society.(5)

Being an academician himself, Weber was also concerned with the fate of the university in this process of modernisation. One outcome of the historical process in this respect, can be illustrated by his own words:

> "We can observe distinctly, that the German universities in the broad fields of science develop in the direction of the American system. The large institutes of medicine or natural science are 'state capitalist' enterprises, which cannot be managed without very considerable funds. Here we encounter the same conditions that are found wherever capitalist enterprise comes into operation: the 'separation' of the worker from his means of production".(6)

Weber's reflections on the fate of Germany's universities (and their professors) should, of course, be viewed from the point of view of the societal and historical context. In the pre-Weimar period German university professors held a crucial position vis à vis the political-administrative structure of society. Thus, according to the historian Ringer, we may perhaps view Weber's observations at least partially as "a mandarin's reaction" to the sudden events of an industrial epoch, which tended to reduce them to a more marginal position in society.(7)

A mandarinated scientist or not, Weber in view of the new forms of social organisation as "practically inevitable", worked out recommendations for the guidance of his fellow social scientists under the new order. In the developmental process of modern society, social science should try to establish an objective stance, a certain aloofness from political events, and it should not set the objectives for politicians but provide them with the appropriate scientific means for attaining the objectives.(8) This was, as Weber saw it, the responsibility of social scientists for the development of modern society.

The ambiguity of Weber in this respect has puzzled later observers. Thus, the Weber-scholar F. Tenbruck argues that Weber's thoughts on the responsibility of science should be interpreted in a more profound sense, and at the same time be viewed in relation to his other work on societal development. The point at issue, according to Tenbruck, is the unpleasant situation of conflict in which modern social science becomes involved: the task of politisised social theory in modern society.(9)

Weber is not commonly counted among the early inspirators of the sociology of science, even though it is clear that his work was one important source of inspiration to Robert Merton's first important work on the historical sociology of science.(10) Besides, hardly any other early sociologist (Marx excepted) has done so much to influence the general macro-theoretical development of sociology, as Weber. Through this influence, the role of Weber for the agendas of science in society was, at least, indirect, but it also contributed to keeping alive the themes of the early sociology of knowledge, as to, for instance, the fate of science and scientific endeavours during its own development (cf., e.g. the works of J. Habermas and H. Marcuse).

The three thinkers now discussed - Marx, Durkheim, and Weber - all offered rather grandiose agendas for sociological studies of society and its features. Their followers soon came, in their respective academic sociological settings, to reject such schemes with a wide range of perspectives as somewhat unrealistic. Ambitions with regard to the relevance of sociology were also subsequently lowered. Instead of offering far-reaching suggestions for the reorganisation of society as a whole, or discussing the broader implications of social science in this respect, sociologists came successively to prefer giving advice on short-term and more narrow issues. This process was, of course, fostered by the increased disciplination and professionalisation of sociology. In this development, the specialities sociology of science and sociology of higher education arose as distinct fields of study. In that respect a discussion of the sociological classical heritage would, however, be incomplete without mentioning the early sociology of knowledge.

The disciplinary heritage: The early sociology of knowledge

The emergence of the sociology of knowledge as a field of study is in fact not easily pinpointed. There is a sociological curiosity as to the problems of knowledge-in-social-context through the whole of Western intellectual history. To become a distinct sociological field of study, sociological curiosity about knowledge in society seems to have needed an inspiring infusion from an idealistic philosophical and cultural context. It was the context of German academia in the early 20th century that was to play that historical role of context. The process of modernisation, following the Industrial Revolution, gave rise to different movements and sentiments among the European intellectuals.(11) Some of these sentiments inside sociology gave rise to a sociology of knowledge. Thus, it was first with the German thinkers Max Scheler and Karl Mannheim that sociology devoted systematic attention to knowledge as an academic sociological issue.

To Scheler the fundamental principle was that the forms of cognition and of cognitive development were always and necessarily sociologically determined. The structure of society thus determined the sociological circumstances, which in turn constituted the developmental conditions for what was perceived as knowledge for a social group and

also the laws distributing knowledge within the group.(12) Scheler's sociology of knowledge was developed as a rebuttal of Comte's view of knowledge as a given system in society. Furthermore, Scheler accused Marx for holding a view of economic overdetermination, and he also criticised Hegelian idealism and Kantianism as a whole. Contrary to what the thinkers of the enlightenment held, Scheler claimed that there was no unity in human nature, and contrary to Comte, he also denied the existence of universal categories of thought. Consequently, Scheler argued, every period of history presented a unique cultural constellation generating a specific array of values and ideas. This culturally founded "ethos" furnished Scheler with a fundamental sociological category with which to explain the forms of knowledge and the origins of concepts.

Scheler, however, did not (as some of Marx' followers came to do) believe any group, class, or social culture, to possess the "ethos" making it possible to understand the "whole context" (only God was capable of doing this, according to Scheler). One could, however, move closer to the truth regarding the structure of the whole by "synthesising" various epistemological views in human culture, e.g. the Eastern and Western ways of thinking. Thus there are certain similarities between Scheler and Durkheim. The outcome of the schemes of both is that the forms of knowledge should be regarded as relative and contextual. To a still higher degree than Durkheim's model Scheler's model, however, lacks the dynamic explaining changes in the forms and content of knowledge other than by, almost mechanical, references to the "cultural context".(13)

Thus, in spite of his sociological imagination, Scheler leaves us with many unanswered questions about the exact nature of the relationship between organised knowledge and the social order. Later on, Robert Merton was to characterise Scheler's sociology of knowledge as immature insofar as it failed to lay down the guidelines for a systematic research programme.(14) Scheler, however, probably coined the concept sociology of knowledge and laid down some guidelines for what was to be developed by Mannheim and others following in their footsteps, as the sociology of knowledge.

Scheler's disciple, Karl Mannheim, distinguished between natural science - especially when based on mathematics - and other types of knowledge. To Mannheim, natural science was a special kind of knowledge, developing relatively independent of social and cultural factors. Otherwise, knowledge was socially based and - as in Scheler's view - heavily determined by the social context and the societal values embodied in social classes.(15) Mannheim seems to maintain that social thinking should, on the whole, be understood from its historical and cultural context. But he assumed that also social sciences are possible to verify. One part of the solution to this contradiction consisted in social thinking being performed by the "free intelligentia" existing beyond the influence of society; another part of the solution lay in accepting subjective and culturally determined criteria of verification.(16)

As far as intellectual debts are concerned, Mannheim was somewhat of a "Weberian" sociologist, in the sense that his conceptualisation of society is rather much in terms of a structure of meaningful social action of individual actors. In his analysis of thought and society, however, another of Mannheim's sources of inspiration was Marx, but Mannheim incorporated only selected portions of Marxist thinking in his sociology of knowledge. These selected portions include one of Mannheim's pivotal concepts: the social positioning of ideological

structures. Ideas are regarded as being related and attached to the social position of the actors, e.g., their class identities. As an exemplification Mannheim regarded the thinking of the conservative 19th century as being related above all to the nobility of that age as a class and interest group. But it is by no means clear which economic interests on the part of the nobility corresponded to and reinforced the ideology of that class. Nor is it easy to draw any general conclusion from Mannheim's analysis of the nobility in 19th century Germany. Thus Mannheim's discourses on the relationship between ideas and class are not easily applicable to a generalised sociological analysis of ideology and society.

The sociology of knowledge, for Mannheim, formed part of his general sociology of culture, where he, however, was mainly concerned with ideological systems in society. As a consequence, his sociology of knowledge became limited - though wide in scope - and mainly discussed the partiality of ideas in the social world. This, together with the shortcomings as to empirical relevance, brought about that his sociology of knowledge, as an approach, was not influential in the construction of a more systematic sociology of science and scientific knowledge.

Mannheim's sociology of knowledge was, however, explicitly adopted as a heritage by Merton in his sociology of science.(17) But it seems to have stopped by that declaration, and from now on different lines of development would follow. During the formative period of sociology of science, Durkheimian, Schelerian, and Mannheimian sociology of knowledge was of little interest to sociologists of science. The post-war sociology of knowledge generally assumed that scientific thinking inside its own domain was independent of its social and cultural context. But in this way the sociology of science presented no problems in terms of institutionalisation; a place for it was found next door to the sociology of knowledge and ideology in general, and at the same time the fundamental assumptions of sociology of science regarding the nature of science and scientific method did not come into conflict with contemporary studies in theory of science and philosophy and history of science. In other words, it seemed as though the sociology of science provided science (including social science) with a "cognitive autonomy" - or, in the language of Weber, with a rational solution to the dilemmas of the sociology of knowledge. Using the terminology of anthropologists, one may also say that sociology "went native" in the halls of academia, in its studies of academic science itself.

Merton's programme for a sociology of science

The sociology of science which was most vigorously instituionalised in the wake of the early sociology of knowledge developed within an academic climate strictly separating the content of science from the context of scientific development. The view of science adopted in neighbouring disciplines that study science, and which constitute both interlocutors and signposts of the sociology of science, are important components of this climate. Although a certain heritage was taken over from Mannheim and the early sociology of knowledge, the emergent sociology of science also took over from the philosophy and history of science the distinction accepted there between "context of discovery" and "context of justification". Accordingly, the relationship between social structure and cognitive system within this structure was barred from the sociology of science and, as a research field at best reser-

ved for the sociology of knowledge or for the anthropologists when studying primitive social structure. Furthermore, the structural-functionalist school in American sociology is of importance for the understanding of the specific design assumed by the Mertonian school of the sociology of science that came to dominate in the 1950s and 1960s.

In the academic upswing of sociology, especially in the post-war years, its cognitive development was, on the whole, quite considerably influenced by the fact that American sociology dominated the scene. Not only because since the early 1930s the most important institutional centres of sociology were located in the U.S., and whatever contributions European sociologists made in the period between approximately 1930 and the early 1960s were overshadowed by the well-organised (and well-financed) projects of their American counterparts. Also, and especially after World War II, the theoretical development, especially along the line of the structural-functionalist school, as well as the development of empirical sociology along the same line came to stand out as somewhat of a model for modern sociology.(19)

As to the American sociology of science, one name stands in focus: Robert Merton. The sociology of science as a distinct subject field in social science really begins with Merton's thesis (in the history of science) on the origins of western science.(20) Merton's analysis was somewhat inspired by discussions on science and society during the 1930s (Bernal, Hessen, et.al.) but at least as much inspired by Max Weber's theory on the relationship between religion and social development. Merton argued that some of society's needs, articulated as "technological needs", provided the incentive for certain aspects of science giving them priority and developing them. Merton maintained that Puritanism was another essential factor. In English Puritanism as a system of cultural norms he saw a necessary precondition for the development of a utilitarian, experimental approach in modern science.

In his later writings Merton draws attention, first and foremost, to the inward ethos of science as a motive force of development, and this is a version of sociology of science which came to exert great influence. Scientific research, as Merton sees it, is an activity following a "highly demanding system of rules", and this system of rules distinguishes science from other activities and it determines the behaviour of the scientific community.(21) This version of the sociology of science acquired a distinct cognitive identity at the end of the 1950s, or, as Norman Storer dates this event, in 1957, when Merton first presented his paper on Priorities in Scientific Discovery. There the system of norms is positioned with the aid of the theory of institutionalised motivation, developed originally by Talcott Parsons. This system of norms overrides the individual person's own motives in scientific work and became, thereby, the "locomotive" of the Mertonian system.(22) Sociology of science since then has produced a host of work in the spirit of Merton (e.g. Hagstroem, J. Cole, S. Cole, N. Storer, and H. Zuckerman, to mention a few of Merton's disciples).

This development might also be viewed with regard to its disciplinary context, i.e. the context of sociology itself. Social order, as a sociological issue had successively become a research issue elaborated rather much aside from the philosophical, ideological and reformist traditions of classical sociology. Mainstream sociology of the 1940s and 1950s did not so much raise questions about the roots and cause of social order or the single best type of such order. Instead the major focus of analysis was shifted to the analysis of the conditions and mechanisms of social order in general and different types of such

orders in particular. The main theoretician of American sociology at that time, Talcott Parsons, contributed to the sociological view of modern society along these lines in terms of structural-functionalism. In a similar way the Parsons' "pattern variables" are based on institutional systems of norms, the Mertonian norms of modern research became related to a structural-functionalist view of society and its scientific institutions. Merton's programme for studies of science and scientists' behaviour thus presented an opportunity for developing empirical knowledge of the scientific community - defined as a well-ordered world apart from society at large - as well as his programme interpreted science as an objective attainment of knowledge in the service of society. No later programme in the sociology of science has been so much in "harmony" with the dominant school in the discipline. - On the other hand, no other school has been so much dominating in sociology, as the structural-functionalist school.

Criticism of the Mertonian programme was to follow, both in the wake of a general critical revaluation of the structural-functionalism in sociology, and within the sociology of science.(23) The criticism of Merton and his theory of norms above all became an important jumping-off point for the revaluation of the sociology of science in the beginning of the 1970s.

Development of the new sociology of science in the 1970s

After about 1970 a number of important developments occurred, internationally speaking, in the field that successively came to be referred to as social studies of science, a field that is rather interdisciplinary in character but with the sociology of science as probably the most productive branch.(24) Since then the issues in the sociology of science have developed considerably, not only in quantity but also as to theories and analytical approaches in use. Whereas most - especially Anglosaxon - work in this field during the 1960s had been based on the approach of Merton, the sociology of science in the 1970s became increasingly concerned with the social processes of knowledge production, and the societal influences on the direction of research. This development has been most marked in European sociology. Parallel to this, the functionalist school of sociology of science still deals with a number of specific topics, in the USA and elsewhere, in a Mertonian spirit.(25)

A key feature of the reorientation has been the recognition that scientific knowledge as such is socially constructed and thereby amenable to sociological analysis, including the cognitive aspects of science, which up to then had been regarded as a privileged domain of philosophers and theorists of epistemology. This development in sociology parallelled a corresponding development within history of science and philosophy of science, and the breaking up of boundaries between the various disciplinary-grounded specialities studying science. The recognition that also the content of scientific knowledge is shaped by social process and structures and thereby possible to analyse sociologically thus occurred shortly after that parts of the philosophical community began to move away from the doctrine of logical empiricism, while some historians of science questioned the strict dichotomy of "internalist" and "externalist" accounts of scientific change.(26) The result was the emergence of more contextual approaches in both endeavours, as well as incentives for a renewal of the sociologists' agenda.

Inspired by the development in the philosophy and history of science - and considerably stimulated by the reading of Thomas Kuhn's book on scientific revolutions - sociology of science in the 1970s thus can be said to have reformulated and expanded both the older sociology of knowledge (à la Mannheim) and the Mertonian sociology of science and scientists to include even the content of scientific knowledge and the relations between cognitive conduct in science and social structure. One account of the development is as follows:

> "Perhaps the most significant event in the social study of science since the emergence of the sociology of science pioneered by Robert Merton in the 1930s is the rise of a genuine sociology of scientific knowledge in the 1970s. For the first time in the history of sociological thought there was a vigorous and systematic effort to subject natural and technological knowledge to the same scrutiny which has long been brought to bear on other systems of beliefs, such as religious and philosophical knowledge or political thought."(27)

This is, however, only part of the story. We observe today not only the resurgence of the sociology of knowledge but the development of a broadly based cognitive science. The development has also encouraged a couple of lines of approaches, sometimes interrelated, and sometimes conflicting, within social studies of science.

(1) One line of development can roughly be termed a <u>new sociology of knowledge</u>, applied to scientific research. Along these lines we have witnessed a revival of interest in the "old" sociology of knowledge puzzle of the social conditioning of knowledge; however, the focus is often on the conditions in a more narrow sense. Following this line of development, we can mention studies by historians and historically oriented sociologists of science, re-reading the past and focussing on how different themes became science - and why some themes did not.(28) The result has also been a number of ethno-methodological studies of how scientists and groups of scientists in laboratories in fact "negotiate" meanings and truths and (collectively) decide what are reliable facts and what are not. Claiming that scientific facts are mainly socially constructed, these studies usually use a combination of economic models and socio-psychological (behaviouristic) approaches to explain the behaviour of scientists. Consequently, these studies focus on, for example, the strategies of scientists toward reputation in the scientific community, and financial calculation toward funding agencies.(29)

Within the new sociology of knowledge there are also other studies that deal differently, and more broadly, with how conflicting interests and social locations affect scientists' judgements and preferences. Thus a theoretically somewhat more sophisticated account of the social conditioning of knowledge production is offered by the "interest model", mainly worked out by social scientists connected to the "Strong Program" in England. "Strong" refers to the main thesis of the program, according to which it is impossible to make any clear-cut distinction between true and false beliefs in science. According to the program, empirical observations and rational arguments do not in themselves prove a scientific interpretation. Hence, by implication, other (social and political) factors must play a supreme role in the establishment of knowledge claims. Thus an attempt is made, using the concept of <u>interest</u>, to link societal objectives to the scientists' behaviour as to choice of theories and scientific explanations.(30)

(2) Rather different from both the ethno-methodological studies and the Strong Programme are approaches that more properly can betermed a political sociology of science. The question here concerns the connection between different types of knowledge and the societal organisation and steering of knowledge production, as well as the overall interactive processes between science and politics in modern society. The term political sociology of science as used here, also covers somewhat different approaches.

In this line we have, for example, research on the social conditions under which science policy affects cognitive development for societal objectives, and there have also been several empirical studies of such "guided" sciences. An early programme along these lines is the "Finalization thesis", developed by a group of social scientists active in the mid 1970s at the Max Planck Institute in Starnberg, Germany. They studied the cognitive development of various scientific fields, using a model derived from Thomas Kuhn. Sciences in their life history go through three stages: (I) an exploratory one, (II) a paradigm stage in which the basic theory of the field is established and developed, and (III) a "post-paradigmic" stage, in which the paradigm is "mature". Whereas in the second stage the internal logic of science directs the development of the field, the first but above all the third stage offers opportunities for steering, where policy-goals become decisive for the inner cognitive development of the field. This last process is called finalisation.(31) The finalisation thesis was soon to be heavily debated and also criticised for theoretical drawbacks, by not having specified carefully enough which cognitive and social factors are involved at each stage of their developmental model, resulting in some unclarity as to the nature of the external steering and its effects.

Recently, more fruitful approaches have developed within the political sociology of science, resulting in a number of studies of the interplay between science and the surrounding society. These studies have, for example, dealt with the role of funding in the development of research specialities, the role of demand from utilisers of scientific knowledge for the development of policy-oriented research, and the impact of interest groups on the development of disciplines and research specialities.(32)

(3) A third type of approach can be distinguished, focusing more explicitly on the development of disciplines and the disciplinary organisation of science. In fact, both before and after 1970, the academic discipline has been one of the axes around which the sociology of science has developed. The disciplinary organisation of science has thus always been of interest to sociologists of science, no matter whether those organisational traits have been taken for granted (as is the case within the Mertonian school), and as a factor with which to explain other aspects of research; or an attempt is being made to explain the inward logic of the actual disciplinary organisation. Especially since 1970 sociology of science has been increasingly concerned with explaining the origins of disciplines and the importance of disciplinary organisation to the development of knowledge, as well as the function of this organisation vis à vis social and political processes.

Several case studies, combining historical and sociological approaches, have thus dealt with the social processes involved in the rise and establishment of disciplines and specialities.(33) Some of tne post-1970 studies have also seriously undermined the assumption of universality in scientific development, an assumption embedded in the

older studies of science. It has thus been empirically illustrated that sciences and disciplines differ considerably as to the methods and procedures they follow in their conduct, and, thereby in their paths of development. Many studies in this regard deal with the natural and life sciences.(34) We have much less systematic knowledge about the social sciences and the humanities, though these disciplines are probably at least as diverse in their development.

A theoretically coherent programme as how to study the disciplinary organisation - including the social sciences - in a comparative perspective has been offered by Richard Whitley. Whitley's approach is based on organisational theory, and in his perspective the (cognitive and social) organisation of scientific fields is central: the form of organisation both constrains and directs processes of knowledge production and validation.(35)

As should be clear by now, the area of social studies of science, in which the sociology of science and different sociological approaches occupy a prominent position, are by no means a homogenous field of study neither in terms of theory nor in terms of the main analytical focus. I have omitted many examples of studies emerging in the last years, not only because of limitations of space, but also because I have wanted to exemplify some unifying themes in the development, rather than the diversity, however promising.(36)

Besides the differences as to scope and main focus of analysis between the different approaches mentioned above, there is also another difference of a more fundamental nature. It has to do with the theory of (scientific) knowledge preferred. Thus, the development has not only led to a revival of many of the themes and questions raised in classical sociology and early sociology of knowledge, concerning the overall relation between social structure and development of dominant ideas in society. The themes also brought the "old" problems and dilemmas with them. The revaluation of the Mertonian sociology of science led to a convinction that the intellectual ideals and cognitive standards are not independent of broader social goals and ideals and should therefore not be studied in isolation from them. This has led to the assumption that some connection exists between the cognitive and the social in the conduct of scientific activities and thereby also in the developmental process of science in society. While this assumption can be said to be one generally shared by everyone working within the field of modern social studies of science, there are important differences as how to view this connection. One line of distinction is thus related to the position taken as how to define the nature of scientific knowledge itself. One extreme position in this matter is taken by the Strong Programme which refuses to distinguish between "true" and "false" beliefs in science and rather tends to see everything as "socially determined". Other social scientists, especially those working along the lines of the political sociology of science, rather prefer to regard science and scientific knowledge as in some sense specific and different from ordinary knowledge.

While such differences obviously have implications for how the studies are made and what conclusions are drawn within the various programmes, what unites the different approaches is the above mentioned assumption that (at least) some connection exists between the social and the cognitive, and thereby contextuality is regarded as important. As I will discuss in the next section of this paper there are, however, some problems as how to develop this awareness and the empirical evidence of the importance of contexts into a more coherent social scientific understanding.

The social process of scientific development

To start with, it is worth pointing out that something might be learned from the development of the sociology of science approaches themselves, of relevance for our general understanding of the development of social science and its specialities. Roughly, the approaches taken in sociology have developed from the more "bold" and perhaps more "interdisciplinary" approaches of classical sociology and early sociology of knowledge, via the more "disciplined" approaches of the structural-functionalist school and its Mertonian programme in sociology of science, to an increased concern again for interdisciplinary approaches, especially after 1970. The development can also be described as an interplay between change and continuity, exemplified by the revival of "old" themes in sociology at a later stage of development, accompanied by revision of the Mertonian school. The revival of old themes, as well as the revision, was also considerably simplified by the development in other disciplines during the late 1960s and early 1970s.

In this respect it is tempting to quote the historian of science, Stephen Toulmin, when he discusses the development of history and philosophy of science, during their revaluation:

> "The image that comes to my mind is that of a kind of folk-dance, which alternates periods of marching and periods of weaving. For a time, the different academic professions march forward separately but in parallel, each in its own special way; then for a time, they join hands and work together on the general problem arising in the area where their techniques overlap; only to break away once more into separate lines, and march along in fresh direction until they are ready to join hands again."(37)

The imagery is certainly nice, even if it is somewhat incomplete as a picture of the development of post-1970 science studies. If we compare the "pre" and "post", science was before the revaluation defined in academic studies as an inherently autonomous enterprise, the cognitive (and "real") nature of which was accessible by means of philosophical analysis only. This assumption implied also an (hierarchical) order as to what was viewed as the legitimate field of inquiry for the different, disciplinary bounded, specialities. The revaluation then led to the development of a separate - interdisciplinary - field of study: social studies of science. The revaluation also meant different things for the different disciplines: only a part of the philosophical community studying science was in fact involved, and the profound effects were experienced in the history of science and the sociology of science, and were most fateful for the last-mentioned field.(38) Perhaps this may indicate that the sociology of science was more in "need" of the revaluation, and therefore more open to its implications?

The inspiration for sociology from the development in the other disciplines was rather selective, and came above all from the reading of Thomas Kuhn and his description of the uneven development of natural science.(39) Kuhn's work was also much more explicit than most of earlier and ongoing examples of revisions in the history and philosophy of science. The message, as received and translated by sociologists, was roughly as follows: Kuhn claimed and illustrated by historical examples that scientific paradigms and dominating theories are never introduced and accepted on purely rational grounds. Instead of the by then traditional view of science, according to which scientific

reasoning and development was mainly based on indisputable facts and universal logic, Kuhn provided a counter-picture showing scientific work to be at least as much based on group-consensus and traditions. This meant another and a more "sociological" style of reasoning about science and development of scientific theories and facts than the Mertonian programme allowed. For the sociologists this line of reasoning was not only more in line with classical sociology and sociology of knowledge, it was also more in accordance with contemporary studies of other systems of ideas and intellectual production. Undermining the notion of science as a simple linear, cumulative process of intellectual development, Kuhn's illustrations of the sociological nature of the scientific community also (at least implicitly) opened up new ways to analyse the relations between societal steering of science, and cognitive development.(40)

It was most likely not the reading of Kuhn (and others) per se that "made" sociology of science change. It is thus reasonable to assume that the revaluation had at least something to do with the "back-ground music played": the development of the discipline of sociology and its shifts of perspectives. The end of the 1960s and the beginning of the 1970s was an era marked by considerable "crise-awareness" in sociology manifested in, for example, booktitles like The Coming Crisis of Western Sociology (Alvin Gouldner, 1971) and La Crise de la Sociologie (Raymond Boudon, 1971). Kuhn's theory of paradigm-shifts in science inspired also some sociologists to analyse their own discipline, and explain its shifts of "paradigms". On the whole, and simultaneously to the development of social studies of science, the theoretical development in sociology has been both immense and diverse. This development did not only involve the revival of marxist thinking in sociology and ensuing criticism and revaluation as to previously strong theoretical approaches (like the structural-functionalist school). It has also signified a theoretical and analytical reorientation that has cut across many previously agreed upon theoretical and analytical distinctions, as, for example, that between macro- and micro-approaches, and between structuralism and more actor-oriented perspectives. The implications have recently (and rather to the point) been summarised as follows:

- The shifts (in perspectives) implied that no institutional order or any structure of social interaction in general, is any longer taken as given, or explained by its need and configuration as shaped, above all, by the extent of its differentiation: nor its functioning according to the contribution of its different parts. Instead there developed a strong emphasis on the process of construction of such order.(41)

Another, probably important back-ground factor, should be mentioned: the develoment of science itself, and the development of the policy of research and higher education, and the direct and indirect effects of this development on the problems and approaches chosen in social studies of science and academia. The increase in the reliance of modern societal development on knowledge - scientific, technical, or professional -, the increased awareness of the problematic implications of technological development, and the often problematic role of experts in contemporary public discourse, are all examples of aspects of modern societal development that call for a broader social science understanding of the relation between societal development and scientific development. Also, the 1960s and 1970s were, as to the policies of research and higher education, an era marked by a strong instrumentalism in policy-thinking as well as a striving towards "structural-rationalisation" of university and state-financed research in most

parts of the Western world. The effects of policy and changing ways of looking at the societal function of science and academia, on the development of science studies might be well worthy of a more detailed analysis.

Anyhow, what I think we might learn from this development is that cognitive change in social science probably takes place inspired by many different sorts of contexts, both inside and outside academia.

We may legitimately assume that social science, as well as science in general, is not only socially conditioned in the elementary sense, that the general structure and developmental stage of society, its culture and policies, the resources put to the disposal of the scientific community, the demand for its results, the institutional structure of science, etc., condition science. Societal factors also enter the scientific process as such in various ways. Firstly, facts and theories are always ambigious and amenable to varying interpretations. Secondly, any scientific theory starts out from conscious or subconscious axioms and hypotheses that conform to judgements concerning relevance or fruitfulness. In these respects the scientific process leaves room for individual choices and preferences, which are not only cognitively but also socially and ideologically determined. Thus, not only the definition of established truth but of scientific truth per se are conditioned not only by the traditional norms of the scientific community but also by social structure, culture and values.

The awareness of the role of contexts in the development of science is, however, precisely one important conquest of the new social studies of science. Thus contextuality is stressed and focused upon in contemporary studies, in the studies of the inner life of research-sites, as well as in studies aiming at explaining the relation between societal aspects and the development of science. At the level of the research community the role of the social context has been keenly illuminated by the ethno-methodological studies of laboratories, and in some of the studies within the Strong Programme. These studies empirically illustrate how scientists make their judgements and choices in scientific matters as social beings, at least as much as they seem to be following epistemologically valid sets of truth-generating rules. One conclusion that can be drawn from these studies is that scientists (like other people) seem to follow commitments, norms, and procedures that are socially and historically contingent.

These studies have, however, their drawbacks and limitations, especially with regard to the role of context in more broad social science terms. The ethno-methodological studies tend to fall short in connecting their models and results with a theory more related to the broader societal context and how, for example, politics (including research policy) enter and influence the inner life of the laboratory.(42) A similar limitation can be said to characterise the Strong Programme when using the concept of interest to link collective societal objectives to scientific ones. The model of the Strong Programme is thus considerably "weaker" when it comes to specify what these factors of influence are and how they operate. The plausibility of the analysis of interests in knowledge production, depends, of course, on what can be concluded as to the exact nature of the connection between interests in society and the more narrowly experienced interests at the level of the scientific community. Lacking this, the main thesis of the Strong Programme, that everything is socially determined, rather tends to mystify the links between modern science and its historical and societal context.

As to studies focusing on disciplinary development and the disciplinary organisation of science, these have also pointed to the role of contextuality in the development of the different fields. Thus, one conclusion to be drawn from these studies is that instead of taking a cognitive universality in the development of scientific disciplines for granted, each field needs also to be understood in its social and historical context, and that patterns of disciplinary change are rather contingent phenomena. As to more theoretical attempts at describing and analysing the disciplinary organisation of science, taking into account both the cognitive and social aspects of this organisation, the work of Whitley has been mentioned. Perhaps somewhat restricted in its "typological" approach, Whitley's agenda, however, points out an interesting way by which to compare processes of knowledge production between different fields of science. In his recent work Whitley thus also opens up for a discussion on how the organisational traits of fields and disciplines in modern science, as well as the content of the areas of knowledge thus delimited, are differently related to societal development. We need, however, more of empirical studies, dealing with such different linkings between scientific fields and their societal surroundings.

It has also been demonstrated with the help of a good amount of case-studies, as well as general social science analysis, especially along the lines of the political sociology of science approach, that modern scientific research is a socially organised activity as well as a product of interdependent and interconnected processes, connecting collective social goals with intellectual procedures and standards. The aspects of this connection, that have been mainly focused on are either those concerning direct societal influences on science (steering and policy), or concerning more indirect influences as, for example, the more or less manifestly expressed demands on knowledge from different spheres of knowledge-utilisation in society. Studies along these lines might probably also gain by taking more into consideration the "micro-milieus" of science, at least as a complementary aspect. Science is, after all, done by people and its execution varies according to norms and procedures expressed as proper by their immediate surroundings and/or as derived from the rules of their disciplines. Furthermore, external influences are "filtered" through the structure of the basic units and the disciplinary self-understanding of the researchers. When this disciplinary self-understanding is rather firm and explicit, it seems to "transform" the external influences and external demands. (A case in point may be the extent to which Keynesian theories on fiscal policies moulded governmental policy since the 1930s.) Other sciences, less firm in their cognitive structure and in disciplinary self-understanding, react differently. Thus, a real comprehensive account of the history and sociology of science in society, has to cover both its inner context and the context of its outward relation to society.

And what about the sociological perspective?

There is no doubt that the new social studies of science, compared to pre-1970s sociological research in this field, have increased our knowledge about the sociological nature of science and scientific work. The findings have also contributed to our overall understanding of the complicated relation between modern science and society that pays for it, and asks for its results to use. Social studies of science is in this regard a rather productive research area but also, as we have seen, a field rather much in flux as to its cognitive development. This state of things has some relevance for the theme of

this conference: how different disciplinary perspectives can contribute to a more comprehensive understanding of the field of research and higher education. One inference may be that sociology of science would profit by devoting some energy towards the refinement of concepts and the generalisation of results won by empirical studies. Also, a combination of results and perspectives within various social science specialities might be beneficial: the sociology of science, as well as the sociology of higher education, might gain not only from political sociology but also from modern social science analysis of the state, analysis of professionalisation and class structure, and modern economic analysis of industrial development, to mention but a few examples of study areas of possible relevance.

As I mentioned previously, we might also need more of empirical studies dealing with the features of different disciplines, specialities and even different higher educational programmes, studies that especially take their contexts into consideration. Especially a comparative approach in this regard may pave the way for several interesting sociological working-hypotheses concerning not only the ways knowledge production and education are organised, but also the ways knowledge develops and how this development mirrors historical and national variations as to cultural heritage and politics, as well as when affected by the market for knowledge in society. Such an approach also needs a combination of perspectives because it has to take into consideration both the relation between the cognitive activities and their organisation, and the relation between the behaviour of research communities and developments in the surrounding society.

One of the outstanding qualities of sociologists - also shared with their close relatives on the academic arena, the anthropologists - is a specific aptitude to problematise the immediate. Probably this talent could be exploited to a greater extent in studies of science and academia. Apart from the ethno-methodological studies of natural science laboratories, we have in fact not so many studies of this kind in the modern sociology of science, which shows somewhat of a contradiction to other endeavours of sociologists. Sociologists have thus been rather keen to explain how the attributes of other people are induced by their immediate surroundings. They have been far less eloquent about how their colleagues and fellow-travellers in academia behave or about how their points of view may be determined by the pursuits of literary and academic careers.

The writings of the French sociologist Pierre Bourdieu, may, however, be mentioned in this context. Bourdieu's studies of the different "arenas" where ideological and scientific production takes place, can also be characterised as a kind of political sociology approach, attempting to bridge the "internal" and "external" contexts. In several works Bourdieu has also analysed the open and hidden rules of academia.(43) From a similar point of view, Regis Debray has offered interesting accounts of the development of the French intelligentia in its own market and cultural contexts.(44) In the field of studies of higher education, Burton Clark's studies of universities around the world, can be mentioned as an example of work where inside-experiences are used in an interesting way.. (see Appendix)(45)

More research will, of course, not per se lead to a more comprehensive social science understanding of science and higher education as social phenomena. One obstacle to such an understanding lies in the domain of theory, and, especially, in the lack of plausible theories of the overall relation between science and society, i.e. theories that might also help putting the various models of explanations offered in more

micro-oriented analysis into a more coherent context. What is more, some of the tools for such a comprehensive understanding most probably have to be developed in the territory between the social science disciplines.(46)

This leads us also to another relevant topic for current social studies of science, as well as for the self-understanding of social science: the development of social science. The following quotation, from a book dealing with the policies of the modern state and their development may illustrate some areas of interest in this regard:

> "Our analysis has suggested that early and sustained access to administratively strategic centers of public policy made it possible for the Swedish economists to produce effective intellectual justifications for state-sponsored reforms in a relatively atheoretical and nonconflictual mode. Do analogous conditions regularily lie behind successful social-scientific contributions to public reforms in capitalist democracies (and other kinds of polities)? Under what alternative conditions do grand theories or conflictual presentations of theories prove more effective, directly or indirectly in the complex processes that lead to transformation in public policies? Still more intriguing. What effects on processes of intellectual innovation, and on intellectual reflection itself, are exerted by the policy successes and setbacks of experts? Would John Maynard Keynes have bothered to fashion the grand theory that gave his name to an epoch - and that inspired such an array of followers and policies ranging across intellectual and political aspects - if he had ejoyed immediate access to centers of public policy making in interwar Britain? If Keynesianism, in turn, had not been born, would the Swedish economists have bothered to declare themselves members of the Stockholm school? What differences would it have made for economic theorising and research, and for public policies and political debates in the postwar period, if these paradigms and schools had not been created?"(47)

This quotation - including its many question-marks - touches some of the most important questions of relevance for our understanding of the development of social science and its relation to societal development. The questions have relevance for our understanding of social science in general, and similar questions may of course be raised about other, less extra-ordinary developmental periods than the one dealt with in the quotation. If we look around and we do not discover anything as exciting as the historical example given above, the reason may be that we view reality with the wrong glasses and therefore need to change or improve our theories and approaches.

NOTES AND REFERENCES

1) The concept of organised knowledge is, as far as I know, first used in sociological analysis by Leslie Sklair in his Organized Knowledge. A Sociological View of Science and Technology, (London: Hart-Davis. 1973). I find the term congenial, but as can also be seen from the context I use it in a somewhat wider sense than Sklair, so as to include the social and disciplinary organisation of science and higher education.

2) E. Durkheim, The Elementary Forms of Religious Life, London: Allen&Unwin, 1976.

3) Berger and Luckmann have given a somewhat different slant to Durkheim's ideas as a cohesive force in modern society. Cf. Berger, P & T. Luckmann, The Social Construction of Reality. New York: Doubleday. 1967.

4) Bloor, D., "Durkheim and Mauss revisited: Classifications and the Sociology of Knowledge", in Studies in History and Philosophy of Science, Vol. 13, Nr. 4, 1982; Barnes, B., Interests and the Growth of Knowledge, London: Routledge & Kegan Paul, 1977; Bloor, D., Knowledge and Social Inquiry, London: Routledge & Kegan Paul, 1979.

5) Weber, M., Wirtschaft und Gesellschaft, Tübingen, 1956.

6) Weber, M., "Science as Vocation", in Gerth & Mills (eds). From Max Weber. Essays in Sociology. London, 1967, p. 131.

7) Ringer, F., The Decline of the German Mandarins: The German Academic Community, 1890-1933, Cambridge, Massachussets, 1969.

8) Weber, op. cit.

9) What Weber "really" means, according to Tenbruck is that:

> "Far from establishing science securily, the institutionalisation of science has led into another sort of 'iron-cage'. The very success of science has led to disenchantment of the world."

Tenbruck, F., "Max Weber and the Sociology of Science", Zeitschrift für Soziologie, Nr. 3., 1974, pp. 312-320. For a related discussion see also, Tenbruck F., "Science as vocation - revisited", in Forsthoff, E. & R. Hörstel (eds), Standorte in Zeitstrom. Festschrift für Arnold Gehlen, Frankfurt: Athenäum, 1974.

10) Merton, R., "Science, Technology and Society in Seventeenth Century England", Osiris. Vol. IV. 1938.

11) See for an account of some of these 'fin de siècle' anti-capitalist movements: Löwy, M., Georg Lukács - From Romanticism to Bolshevism, Norfolk: New Left Books, 1979. Esp. chapter 1.

12) Scheler, M., Problems of Sociology of Knowledge. London: Routledge & Kegan Paul, 1978 (First published in Germany, 1924)

13) Scheler, op.cit.

14) Merton R., "The Sociology of Science. An Episodic Memoir", in Merton, R. & J. Gaston (eds), The Sociology of Science in Europe, Carbondale: Southern Illinois Press, 1977. See also Merton, R., "Paradigm for the Sociology of Knowledge", in Merton, R., The Sociology of Science. Theoretical and Empirical Investigations. Chicago: The University of Chicago Press, 1973, pp. 7-42 (First published as "Sociology of Knowledge", in Gurwitch & Moore, Twentieth Century Sociology. New York: Philosophical Library. 1945, pp. 366-405).

15) Mannheim, K. Ideology and Utopia, London: Routledge & Kegan Paul, 1954; Mannheim, K., Essays on the Sociology of Knowledge, (edited by P. Kesckemeti) London: Routledge & Kegan Paul, 1972.

16) The socio-cultural climate in which Mannheim, influenced both by Weber and Marx, developed his theses of the sociology of knowledge also included the attempts made by the Vienna circle (Carnap, Schlick etc.) to emancipate modern science from its cultural and social influence, with a view to establish an objective foundation of scientific knowledge. The Vienna circle, accordingly, contended against what they regarded as different relativistic views of science. Mannheim seems to have been not entirely happy, although somewhat accepting the ratio of these attempts. Thus the problem of the sociology of social scientific knowledge, for Mannheim was also disciplinary by nature, as also can be read from his posthumously published Strukturen des Denken. English translation: Structures of Thought, London: Routledge & Kegan Paul, 1982.

17) Merton in Merton & Gaston, op.cit.

18) Cf., for example Marcuse, H., One-Dimensional Man, Boston: Beacon Press and Habermas, J., "Erkenntnis und Interesse", in Technik und Wissenschaft als Ideologie, Frankfurt, 1968.

19) For the development of Swedish sociology, taking its first steps in the late 1940s, and from the beginning very much influenced by the American development, see: Fridjonsdottir, K. (ed) Svensk sociologi, Stockholm: Carlssons Förlag, 1987.

20) Merton (1938).

21) An early version of the norms and their sociological implications for scientific work, as seen by Merton, is in Merton, R., "Science and Technology in a Democratic Order", Journal of Legal and Political Sociology, 1942, pp. 115-126.

22) Or, in Storer's own words:

> "Merton had already outlined the normative structure of science and had suggested how the four norms interact with each other in a functioning whole. But there was no clear distinctive source of 'energy' in the system - no sense of why it should 'move'. The formulation was in a state description rather than in a process description. It was as if someone had described the physical construction of an electric motor but had not brought to it a clear concept of electricity; one could see that it might do, but could not understand why it should go round and round",

- is Storer's account of how it was before the theory of the institutionalised motivation was put in. Storer, N., "Introduction" to Merton R., (1973), p xxiii. Cf. also Merton's own account in "Priorities in Scientific Discovery". Ibid, pp. 286-324.

23) A now somewhat classical illustration of "counter-norms" in scientific conduct, is in: Mitroff, I., The Subjective Side of Science, Amsterdam: Elsevier, 1974.

24) See also for accounts of this development: Whitley, R., "From the Sociology of Scientific Communities to the Study of Scientists' Negotiations and Beyond", in Social Science Information. Vol.22, Nr. 4/5, 1983, p. 681-720; Knorr-Cetina & M.Mulkay, "Introduction: Emerging Principles in the Social Studies of Science", Knorr-Cetina, K. & M. Mulkay (eds) Science Observed, London: Sage Publications, 1983. pp. 1-17; Mulkay, M. & V. Milic, "Sociology of Science in the East and West", Current Sociology, Vol. 28, Nr. 3, 1980.

25) For example, studies of the reward system of science, the formal and informal organisation of science, including the communication patterns, etc. See also for (somewhat out-dated) overviews: Mulkay & Milic, op.cit.

26) Lakatos, I., "History of Science and its Rational Reconstruction" in Buch, R. & R. Cohen (eds), PSA 1970: In Memory of Rudolf Carnap. Boston Studies in the Philosophy of Science, Nr. 8, Dordrecht: Reidel, 1971; Shapin, S., "History of Science and its Sociological Reconstruction", History of Science, Vol. 20, 1982. For an overview of the discussion in philosophy, see Suppe, F. (ed): The Structure of Scientific Theories, University of Illinois Press, 1977.

27) Knorr-Cetina & Mulkay, op.cit., p.2.

28) See for example. Barnes, B. & S. Shapin (eds) Natural Order. Historical Studies of Scientific Culture, London: Sage Publications, 1979.

29) Latour, B. & S. Woolgar, Laboratory Life, London: Sage Publ., 1979; Knorr-Cetina, K., The Manufacture of Knowledge, Oxford: Pergamon Press, 1981.

30) See, for example, Barnes, B., Interests and the Growth of Knowledge, London: Routledge & Kegan Paul, 1977, - and note 4 above.

31) Böhme, G., et.al., Die gesellschaftliche Orientierung des Wissenschaftlichen Fortschrittes, Frankfurt: Suhrkampf, 1978; van den Daele, W., "The Political Direction of Scientific Development", in Mendelsohn, E., et.al. (eds), The Social Production of Scientific Knowledge, Dordrecht: Reidel, 1977. See also for an overview and discussion, Rip, A., "A Cognitive Approach to Science Policy", Research Policy, No 10, 1981, pp. 294-311, and Mulkay (1980), p 103ff.

32) Much material has been published in periodicals and specialised books. The periodical Social Studies of Science contains much material that is relevant to get a picture of the development of the field as a whole. Also, the Yearbooks of Sociology of the Sciences should be mentioned. A forthcoming yearbook deals especially with the influence of interest groups in society on the development of science (Yearbook, 1987, Vol XI), for example, on the university-industry relation: Blume, S.S., "The Theoretical Significance of Co-Operative Research", and on the development of social science: Fridjonsdottir, K., "Social Change, Trade Union Politics, and the Sociology of Work", and Wagner, P., "Social Sciences and Political Projects: The Emergence and Demise of Reform Coalition between Social Scientists and Policy-Makers in France, Italy, and West Germany". The work of Wagner is also part

of a research project, aiming at explaining the development of policy-oriented social science in a comparative perspective. See: Thurn, G. et.al., "The Development and Present State of Public Policy Research. Country Studies in Comparative Perspective", Berlin: Wissenschaftscentrum, 1984 (mimeo).

33) Lemaine, G. et.al. Perspectives on the Emergence of a New Discipline, 1976; Edge, D. & M. Mulkay, Astronomy Transformed, New York: Wiley, 1976.

34) Graham, L. et.al. (eds), Functions and Uses of Disciplinary History, Dordrecht: Reidel, 1983; Horgens, L., Patterns of Scientific Research, Washington: ASA, 1975. According to the assumption of universality in scientific development, the social sciences have been viewed (also by Kuhn) as pale, but "unripen", replications of physics. See for different discussions on the organisation of social science, Whitley (1984), and Whitley, R.D., "The Development of Management Studies as a Fragmented Adhocracy", Social Science Information, No. 23, 1984, pp. 775-818. For studies of the different cultural features of disciplines, see: Becher, T., "The Cultural View" in Clark, B.R. (ed), Perspectives on Higher Education, Berkeley: University of California Press, 1984, pp. 165-198, and also the contribution by Becher to this volume.

35) Whitley (1984).

36) As, for example, analysis of scientific discourse, by M. Mulkay and others. A recent book by Mulkay is, The Word and the World. Explorations in the Form of Sociological Analysis, Allen & Unwin, 1985. See also Nigel Gilbert, G. & M. Mulkay, Opening Pandora's Box. A Sociological Analysis of Scientists' Discourse, Cambridge University Press, 1984.

37) Toulmin, S., "From Form to Function: Philosophy and History of Science in the 1950s and now", Daedalus, Nr. 1, 1978, p. 160.

38) Cf. also the discussion in Shapin (1982).

39) Kuhn, T., The Strucuture of Scientific Revolutions, Chicago: Chicago University Press, 1962.

40) According to Barry Barnes:

> "...it is worth noting that Kuhn made one of the fundamental contributions to the sociology of knowledge. It fell to him to provide, at the time it was most needed in the 60s, a clear indication of how our forms of natural knowledge could be understood sociologically. This encouraged the empirical studies of scientific cultures which were then beginning in the social sciences. And it inspired a revived awareness of the constitutively social character of knowledge generally - an awareness which had faded away in the post-war period. This is why a discussion of current views in the sociology of scientific knowledge can be developed from an examination of Kuhn's work, even though Kuhn himself is not a sociologist."

Barnes, B., T.S.Kuhn and Social Science, London: MacMillan Press, 1982, p.x.

41) Eisenstadt, S.N. & H.J.Helle (eds), Macro Sociological Theory. Perspectives on Sociological Theory, London: Sage Publ., 1985, pp. 1-2.

42) Cf. also the discussion in Collings, H., "Stages in the Empirical Programme of Relativism", Social Studies of Science, Nr. 11, 1981, pp. 3-10, and, Whitley (1983).

43) Bourdieu, P., "The Specificity of the Scientific Field and the Conditions for the Progress of Reason", Social Science Information, Vol. 14, Nr. 6, 1975, pp. 19-47; Bourdieu, P., Les héritiers: Les étudiants et la culture, Paris: Les Editions de Minuit, 1974; Bourdieu, P., Homo Academicus, Paris, 1985.

44) Debray, R., Le pouvoir intellectuel en France, Paris, 1979.

45) Clark, B.R., The Higher Education System. Academic Organization in Cross-National Perspective, Berkeley: University of California Press, 1983.

46) For an interesting attempt at reconceptualisation in social science, a.o. bridging the macro and micro (cf note 39), see: Giddens, A., The Constitution of Society. Outline of the Theory of Structuration, Glasgow: Polity Press, 1984.

47) Weir, M. & T. Skocpol, "State Structures and the Possibility for 'Keynesian' Responses to the Great Depression in Sweden, Britain, and the United States", in Evans, P.B. (ed), Bringing the State Back In, Cambridge University Press, 1985, p. 148.

APPENDIX

Burton R.Clark: The Higher Education System. Academic Organisation in Cross-National Perspective

To set forth the basic elements of higher education in the contemporary world, as well as illustrating its national variations, is a huge task, and there are few social scientists in the world today that could be expected to manage to do it. Burton Clark, an author of several books on academia and higher education, is one of them. Professor Clark is not only a prominent scholar in the field of higher education studies, he is also a person with considerable practical experience from higher education organisations around the world. Reading this book, one gets the impression that Burton Clark now tries to sum up his experience in a systematic way. The book is above all meant as a contribution to a comprehensive understanding of university organisation. According to Clark himself, the purpose of the book is:

> "...to improve the state of the art (i.e. within sociology of higher education) by detailing systematically how higher education is organised and governed. The approach is twofold: to set forth the basic elements of the higher education system, as seen from an organisational perspective; and to show how these features vary across nations, with fateful effects." (p.2)

Comprehensive treatments of social phenomena, especially when they are, as in Clark's treatment of the higher education system in the world, pressed into a compact form, are, of course, always open to criticism. It is hard to combine the simplicity and the convincing beauty of the unified theme with the necessary details and depthness of the comprehensiveness. This has some relevance for the criticism that can be raised against Clark's book. Let me, however, state here from the beginning that this book is exceedingly well written. The chapters are commendably lucid in presentation and logical in organisation. The problems dealt with in the book are grouped around different themes, like the disciplinary work organisation of academia, the value system, the authority structure, the system of coordination, and so on. In all this the author proceeds in a systematic way, guided by his conception of what traits in the university as an organisation are worth discussing, and how these traits are related.

The starting point is a rather general discussion of the organisation of academic knowledge. Universities handle knowledge in bundles, such as disciplines and educational programmes. People who are competent in corresponding areas are actively concerned with organising and putting together these bundles. The university organisation as a whole is in turn responsible for certifying the competence and intellectual value of the bundles, as well as guaranteeing the best circumstances for the advancement of knowledge within the various disciplines and programmes.

The key feature of the university as a work organisation is the basic academic task structure. In the section on academic work, Clark thus points to the fundamental importance of the disciplinary structure for the academic organisation as a whole:

- The core membership unit in academic systems is discipline-centered.

- Each disciplinary unit within the enterprise has self-evident and acclaimed primacy in front line task.

- The characteristics of core membership groups affect everything else of importance in the organisation (p.33-34)

Due to these circumstances, universities as system differ, according to Clark, from all other organisations in society - which at the same time accounts for the immense importance of the basic units in academic organisation.

Consequently, the disciplinary structure creates organisational problems peculiar to the university. Authority is primarily vested in disciplines and hence highly fragmented. The university is, however, maintained by a public authority for purposes of education, professional training and the general advancement of knowledge. This requires a unified enterprise, differently organised in different societal systems; thereby the political and administrative "culture" of different countries is mirrored in the way their universities are administrated. The differences in this regard, most profoundly dealt with in the book, are those between the European system and the Anglo-Saxon (above all the U.S.) system, however many illuminating references are made to e.g., the Soviet and Japanese university organisation. When discussing work organisation of the university (in chapter 2), and, traditionally rooted as well as politically created, administration patterns (also in chapters 4 and 5), Clark also considers how higher education systems can be valued, according to their organisation. One conclusion that can be drawn from this discussion, is that the U.S. system in

general is more productive for the advancement of knowledge, as well as it provides the best surroundings for higher education.

There are other features of society than administration and governmental steering that might be expected to affect the development of universities and their disciplines and higher educational programmes. One of these features is the market and its "pulling-effect" on the development of knowledge and the training of the highly educated. This, however, receives a rather limited treatment in the book, even in the chapter (5) on the societal integration of universities. This is perhaps not so surprising. The main theme of the book is the internal integration of the university; the systems are seen as reacting to external forces - be they market pressure or governmental steering - but these forces are regarded as heavily molded and transformed by the organisation according to the basic logic which stems from the particular character of academic work. In this the systems are seen as more or less successful - depending, in turn, on how (well) they are organised. This view can certainly be debated, and I will take this up again in the discussion below.

Regardless of their national differences as to organisation, the disciplinary structure is thus seen as pulling together a strong and craftlike community of (disciplinary) interest. Another factor of coherence is more cultural in character: the differentiated systems of disciplines are also held together by culture, by common ideology, norms and values. The difficulties in the organisation of the academic enterprise can thus to some extent be rectified symbolically. Not only do disciplines create their disciplinary cultures and universities their (national and international) cultures, but students and administrators also have their own subcultures within the system. Culture creates identity and solidarity, and plurality of cultures is not seen as problematic by itself, only when it is accompanied by clashing values. While a basic set of values and norms - social justice, competence, and liberty - unites the academic community, the academicians, the students, the administrators, and even the public can from time to another be internally divided. What helps to legitimate the academic belief system is when, at least, certain of the academic beliefs have their principal source and attainment in the national system as a whole. (Chapter 2, see esp. p. 95 et seq., and also chapter 7).

After a discussion, with a lot of examples, in chapters 4, 5, and 6, on the different ways the system of higher education and its work are organised, changed, integrated and sustained, there follows a chapter dealing especially with the conflict between different values, social justice, the pursuit of competence and excellence, and liberty in higher education. This chapter and the final chapter are rather normative (also presented under the heading "Normative Theory"), opening with a presentation of six ideas about the optimal implementation of these values. Each proposition is thereby supported by a discussion of the undesirable consequences of alternative actions by the policy.

Discussion

Burton Clark's book is in many ways a good and important book. It provides ideas and information on many issues of interest today for research, and it is above all a good contribution to the general discussions of the problems and future of higher education and university based research. What makes it especially nice to read is that the author so generously offers his own insights and experiences from a variety of scenarios around the world, where higher education is

conducted. In other words, this book is both absorbing and even convincing reading, even in its, in my view, somewhat exaggerated analytical simplifications. The book is also impressive by virtue of its wide, comparative approach and the attempts it makes, with reference to a wide-ranging material, to characterise a number of fundamental and, as Clark himself maintains, almost universal features of the academic system, features which at the same time are said to make the university a unique organisation in society.

A wide approach of this kind, with its high level of aspiration as regards capturing the fundamentals of the main institution of knowledge in modern society, wherever they are situated, is liable to be superficial in at least some respects. Thus one shortcoming concerns the empirical material on which this book is built and the way in which the material is used. The book <u>draws</u> material from all over the world but <u>deals</u> mainly with the industrial and educationally advanced world in the West. The rest of the world is mainly used as a point of reference, especially when its higher education systems seem to show traits quite similar to - but also quite different from - the systems in the U.S. and Western Europe. This "cultural bias", sometimes nicely compensated for by references to the "role of culture" (as for example to the Nipponic samurai tradition and its implications for the inner life of Japanese academia), need perhaps not be so serious <u>if</u> we accept that the book is above all concerned with the systems of the Western world, their variations and problems.

But this in itself is quite a considerable perspective, and it is also one in which Clark's book can be seriously criticised. To begin with, I would like to raise one point of criticism, having to do with the concept of <u>knowledge</u>, especially disciplinary knowledge, in the book rather much "taken for granted" and not problematised. I am aware that my criticism has to do with the differences with regard to theory of knowledge, between me and Burton Clark, his being rather much that of viewing scientific development as "autonomous" and immune to external pressure and influence. However, in a book which is, in fact, based upon a strong assumption regarding the overall importance of disciplines, one could reasonably expect to find some discussion of what makes up the disciplines in terms of theory or sociology of knowledge.

Another problem has to do with the explanation, given in the book, of the prime move of changes in disciplinary organisation as well as the propelling motor for an autonomous development in academia. The concept of <u>interest</u> is used by Clark as the main explanatory factor. In the words of Clark himself:

> "It helps to have a single idea to fall back on as we move through a variety of topics, trying simultaneously to be far-ranging in international coverage, reasonably true to reality in various national settings, conceptually parsimonious, and hence somewhat confusing and even contradictory. That underlying idea is <u>interest</u>. The division of labor in the academic world, as elsewhere, is a division of human commitment. The work commitments have related orientations. Commitment and orientation., work and belief, compose interests, a composite of the material and the ideal. The interest groups that are thereby generated attach authority to themselves and seek modes of integration that are congenial. In turn, the powered structure of interests becomes the primary determinants of change in the system. It also determines to a considerable degree how, and how much, the system comes to express certain broad social values." (p.10)

This discussion appears in the preface of the book, and is not followed up later on, only referred to. Thereby the underlying idea of interest, partly hangs loose in the air, partly becomes implicitly identical with the professional interests of the disciplinary researchers themselves. I am inclined to, at least partly, agree with Clark, because I am convinced that interest in this meaning has importance in the conduct of academic work. I also appreciate the beauty of a "single idea to fall back on" - (sociology would be still nicer to do if we had more of that!) However, much of the criticism that has been raised against the structural-functionalist based Merton's systems of norms as the main motor in scientific work (previously discussed in the paper), is relevant also for Clark's model. The criticism of Clark's explanation by interest can, furthermore, take up the following: firstly, it is reasonable to suppose that most organised knowledge is produced, broadly speaking, for some "purpose" (and not merely for detached world-renouncing contemplation), and this purpose is rather often external to the institutions of higher learning. This holds, by the way, also for the different ways of organising (and categorising) different types of knowledge - from the tripartiate organisation of knowledge in the middle ages to the disciplinary organisation in the universities today. That is, they do in some way mirror the organisation of society, and not only the pursuits of interests of their practitioners. I certainly miss this aspect from Clark's book.

Secondly, Clark's emphasis on interest as the unifying factor of academic activities cannot explain why the practitioners of university science at least sometimes strive to develop scientific knowledge in general, instead of only safeguarding their disciplinary and professional interests. If "interest" is meant to refer also to things like the "scientific ethos" and idle curiosity, (also across the disciplines) the concept seems to me to be too loose to be effective as an instrument of explanation and understanding.

A third critical point regarding the concept of interest as an explanatory factor, as Clark uses it, concerns the picture we get of university in modern society. By so heavily emphazising the "inner dynamic" of the system, the development and products of university science and education are seen as largely effects of this dynamic, and the system itself as developing almost "automatically", somewhat intimating its requirement to the whole social system, but also successfully resisting its influence on knowledge production. This may be a nice picture (especially for the practitioners of science and education) but I think it is at least an incomplete picture. While it may be fruitful for analytical purposes to treat the university as a somewhat autonomous element in the total society, I think that in order to understand its structure and basic features we have also to relate the analysis more clearly to the broader context, for instance, market and politics in modern society.

As mentioned above, the market as a steering factor is not dealt with specifically in Clark's book. Reference to influences from societal and political factors are however mentioned, as for instance concerning social science and humanities and their alleged (special) openness towards political affairs:

> "Disunited within their own fields on grounds of basic approach, theory and methods, social scientists and humanists are (thereby) rendered more vulnerable to specific political

> views or world views brought into one's work from outside sources. Sociology, political science, and history are relatively open in this sense, with, in comparison, mathematics, physics, and chemistry operating more like closed cultural systems." (p. 80-81)

One cannot deny the "openness" of the social sciences towards societal affairs (it is too obvious), during the course of these disciplines' development. I guess that the humanities, at least part of the humanities, may be said to show a similar ppennes toward the ideological development of society. But openness also exists in the natural and life sciences and this is especially visible in their later development.

Let us take one example: biology, and its relation to the market of biological products. Some twenty years ago, private industry had little to do with gene research and related research in biology. Then, in the late 1960s, molecular biologists started to introduce an array of techniques that could be exploited for making such new gene products as antigen for hepatis-B vaccine, the antiveral agent interferon, and human insulin. Each of the gene-splicing techniques had thus generated a lot of information about the structure and function of genes in higher organisms. Taken together, these techniques also opened up possibilities of using it in profitable production in industry. The results gained in research thus made the disciplines and specialities involved rather interesting as cooperative partners for industry.

In the academic community, the ensuing contacts between university and industry had a number of (probably unforeseen) consequences for state-financed, as well as for private-or-both financed, universities, not only for research but also for higher education. Scientific research as a process, depends on a constant volley of ideas and technology. An integral part of basic university research is the process of learning, so is too the training of new scientists. Traditionally, university learning is intrinsically nonproductive, the tangible fruits of university education, for instance the Ph.D, are not commodities to be sold or patented. Neither are research results, according to the same traditional view. What happened in the course of the events, starting approximately in the late 1960s and early 1970s, was that research in this field was increasingly commersialised. Not only that the research frontier of gene-research moved from university to industry, also, the buying-up of new-bred scientists by industry, is reported (for instance in Sweden) to risk the university education in this field. The strong interests of industry in this field are also definitely affecting the cognitive priorities in university science.

What surprises me, given that the university-industry relationship belong to the topics most intensively debated upon in the discussions on the fate and future of university, is that this problem-complex is hardly mentioned in Clark's book. Especially since I think Clark's analysis would have been more interesting if it had included such external aspects of interests.

Among the concepts Clark brings into his description of the university systems around the world, are academic culture and disciplinary culture. I think that these concepts can be fruitfully used also in an analysis of the relations between the university systems and the external societal processes. Questions like how do different academic cultures and different disciplinary cultures react toward pressure from external forces and interest groups, like the market or polity,

are certainly relevant for our understanding of the university in modern society. As Clark also mentions, the academic culture is the complex outcome of various processes and policies, inside and outside academia. While Clark has, to my mind, pointed to something important for our social science understanding of university, his treatment of the academic and disciplinary cultures are, however, rather descriptive. His analysis might have gained if he had also dealt in depth with the development of these cultures in their interaction with their environment. Through such an analysis, his explanation by interest, as the main motor in academic development, might also have changed somewhat toward a broader - and more complicated - conception.

Rolf Ohlsson:

THE HIGHER EDUCATION SYSTEM IN A TIME PERSPECTIVE

Introduction

The Higher Education System by Burton Clark is an extremely interesting and impressive work. To my knowledge - although it is quite limited in this particular area - Burton Clark's work ventures into a field of research, which so far has almost been a terra incognita. The author tries to grasp the common and basic elements in the higher education system by using the latest research in analyses of higher education in a large number of countries. In addition, an abundance of information is presented about the structure and development of the higher education system in these countries. As far as I am concerned I am particularly impressed by the analysis of the American system and for the first time I believe myself to have arrived at a certain insight into how "the most disorderly of the advanced systems of higher education" functions.

It is obvious that the work has been undertaken by a person with long academic experience who is in possession of a sociological methodology which he fully knows and understands to use in a most brilliant manner. In addition, he has an eminent ability to systematize a complicated and disparate background material, and has obviously spent many years to complete the work. It is, therefore, unfair to put this book into the hands of a rather young scholar, who has come into educational research by the back door, and who prefers to use economic historical methods and concepts. However, I think it is impossible to uncover the structures and processes underlying academic organization - which Burton Clark claims to do - without giving due deference to economic and historical aspects in the analysis. In my opinion these aspects are conspicuously lacking in the book and the following comments should be seen in this perspective.

I have also been asked to write about the contribution my discipline - economic history - can make to the understanding of higher education and research. Needless to say that would require much more space than I have been allotted, to be meaningful in a deeper sense. But as a short declaration of program I would suggest the following. Firstly, higher education and research must be seen as an integral part of a composite and complex societal dynamic process. However, at first it is not possible to isolate any simple cause-effect relationship in this process. Thus, a theoretical and deductive approach is needed to acquire a deeper understanding of the relationship between higher education and research on the one hand and societal structure and change on the other. Secondly, some very important constituent elements in this dynamic process are of economic nature: the living standard in the society, technological level, the structure of industries and jobs, relative wages, the situation in the labour market, demographic structure etc. All these elements are closely related to higher education and research. Thirdly, in the economic literature of the last three decades several theories and hypotheses on the function of education and its economic role in the society have been advanced (education as investment, education as consumption, education as a signalling, screening and as a sorting device). It is in many cases natural that an economic historical study of higher education and research is based on these theories and hypotheses. Nevertheless, it

is self-evident that these hypotheses must be modified when analysing a specific aspect of higher education and research in a particular country during a particular time period. Fourthly, for quite natural reasons an economic historian uses a historical perspective to get at the underlying causal relationships. Too narrow a time perspective has the inherent weakness that one focusses on the analysis of less relevant factors, which only have a short-term explanatory value. By using a long-term perspective, we can emphazise not only continuity and general aspects, but also discontinuity and specific aspects in the development process.

Of course, these are general aspects of a methodological nature and not very precise. But it goes without saying that the contribution any discipline can make to the understanding of higher education and research must be related to a specific problem. Economists and economic historians have analysed several important aspects of higher education and research. But to my knowledge no economic historical study exists, where the central issue has been the problem, which Burton Clark discusses in his book, namely the organization of higher education and research. Thus, the subsequent comments are based on a methodological approach, which is completely different from Burton Clark's internalist perspective. Furthermore, the comments must primarily be seen as an alternative way of analysing the higher education system and as suggestions and hypotheses for further research.

A methodological remark

I agree with Clark that few studies have been undertaken, which stress the specific features of the higher education system. Most studies have looked upon this system as comparable to any other organization. Usually, they have also been limited to a single country.

On the other hand, I think there is a danger in emphasizing too much that the higher education system has characteristics different from other organizations. Of course, the higher education system has some specific features as it is a social structure for the control of advanced knowledge and technique, producing a very specific kind of goods and services. But it also shares a lot of common features with other organizations due to the fact that all organizations including the universities have been influenced by the same changes in the socio-economic structure. I have a feeling that the dissimilarities vis a vis other organizations are stressed too much, while the similarities which are likely to be quite strong almost disappear in Clark's analysis. Superficially, the higher education system may seem to function in a totally different way, but in reality it shares its basic features with other organizations.

A more theoretical approach would probably have given somewhat different results. To me it seems natural to start from a set of theories of how organizations function during different time periods. On the basis of these theories hypotheses could be formulated and tested against the "historical reality". By using this more deductive approach not only common and general aspects, but also specific features in the higher education system could be identified in a more systematic and distinct way. I will come back to this point later.

Higher education and societal change

In Burton Clark's presentation the higher education system is more or less analysed as if it existed and developed within a vacuum. It is mainly influenced by its own endogenous factors of determination and thereby it is given an independent and autonomous role to play in societal change. On the other hand, Clark regards the system as comparatively uninfluenced by the economic and social structure of the society as a whole.

In my opinion, the higher education system cannot be analysed in this simple and schematic way regardless of whether we are concerned with its way of dividing work, of promoting belief or of distributing authority. It goes without saying that economic organization, the varying roles played by the state in different countries, the strength of different interest-groups, national characteristics etc must be included in the analysis to a much larger extent, if we are to arrive at a better understanding of how the higher education system works. More generally, I think it is possible to distinguish, on the one hand characteristics which are common for all countries and which have led to a similar development of the function of the system, and on the other hand, national characteristics which explain many of the observed differences. Furthermore, in my opinion it is the absence of an approach with a stronger linking of the system to the surrounding social and economic structure that explains why Burton Clark's presentation has a character of description rather than of analysis and explanation.

Higher education in a time perspective

This limitation becomes particularly apparent when also the time perspective is missing. In the beginning of this century the higher education system functioned in a totally different way as compared to its function in present times. It is my firm belief that neither the general nor the specific features of the present system could be understood unless due emphasis is given to the historical development. Also in this context certain general and common patterns of development in the world economy have led to a tendency towards convergence of the educational systems in different countries. Some of these patterns are well known:

(a) a rapid technological change, in a wide sense, which have altered the functions of all organizations and changed the demand for labour and the situation in the labour market;

(b) a rapid growth of per capita income during the major part of the 20th century, which, on the one hand, made it possible to raise the educational capacity and, on the other hand, led to an increase in the individual demand for education;

(c) the demographic development which has led to a shift from quantity to quality of children, which perhaps is the major factor behind changes in the demand for post-compulsory schooling during the 20th century (see below); and

(d) the growth of the tertiary sector in the economy which has drastically increased the demand for highly educated labour.

All these factors have in a dramatic way influenced the interest-group struggle within the university system, which according to Burton Clark

is the heart of the dynamics of the system. But these factors have also been underlying the steady process of differentiation. Thus, the transformation that has gradually been going on during this century can hardly be understood and analysed unless these fundamental changes in the economy are taken into consideration.

It can be argued that the core of the matter is size. All the above mentioned factors have resulted in an enormous increase in the numbers going to universities. This can be exemplified by the Swedish experience. In 1923 1,5% of those who were 20 years old sought entrance to university studies, while the corresponding figure in 1968 was 23%.

In this context I think a comparison with the fundamental changes that have occurred within business organizations, can be useful. In the 19th century traditional enterprise, the volume of activity was not large and owners had no difficulty in administering their enterprises. It remained small and personal and in nearly all cases it was a family affair. Artisans, craftsmen, shipbuilders, distillers, who relied on the labour of apprentices and journeymen found for example the old methods of accounting completely adequate. The same was true of manufactures who expanded production by adopting machinery. The simple machine required neither heavy investment nor a large work force. Very few enterprises in either production or distribution had acquired an internal organization as complex as a single operating unit of the many that make up a modern business enterprise. Then, in the course of time, technological innovation, the rapid growth of population and expanding per capita income made the process of production and distribution more complex and increased the speed and volume of the flow of materials through them. The new technologies and expanding markets thus created for the first time a need for administrative coordination. To carry out this function entrepreneurs hired salaried managers needed to administer the enterprise (the managerial revolution). As technology became more complex and more productive, as markets continued to expand, these managers assumed command in the central sectors of the economy.(1)

The changes in the university organization during the 20th century show, in my opinion, several parallel features to this fundamental shift in the capitalist society from traditional capitalism to managerial capitalism. Technological change, growth in per capita income, demographic factors and changes in the job structure have resulted in an enormous expansion. As a response to this, the higher education system had to find new forms of organization to attain a more efficient resource allocation. The old "master-apprentice" relationship had to be modified, and new ways of dividing work, of promoting belief and of distributing authority had to be found. One conspicuous feature in this development was an increasing dominance of bureaucracy. Of course, it is another story if the ex ante perceived organizational changes to attain a more efficient resource allocation also have been effective ex post.

At this point I would like to emphasize that this comparison with the changes in the capitalist society primarily serves as an example. But it clearly illustrates that an externalist approach will identify other explanatory factors than an internalist approach. Obviously, national particularities in the historical development must be considered when the specific features of the higher education system of a certain country are analysed. Although some of the basic underlying structural changes in the society are general and common for many countries, the new forms of organization have to develop within a

specific cultural and social framework. It is not just by chance, that the Swedish system is highly bureaucratic.

I would like to make a final comment in this context. Most countries are not on the same level of development at the same point of time. Thus, one cannot be certain to reveal the underlying and common features of the structure if the higher education systems of different countries are analysed from a cross-sectional perspective. To be able to reveal the common features of the system, a more dynamic approach than Burton Clark's is necessary.

The economic function of higher education

Knowledge and the content of education are relative rather than absolute concepts. I believe it is very important when analysing the organization of higher education and research - the underlying "academic forces of production" and the struggle between different interest-groups - to take into account what economic role education plays in the society, for example in the labour market. It is equally important to take into consideration how the services produced by the universities are appraised by the surrounding society, and how individuals react to these appraisals when they choose education. This is a part of the story, which I find important and which I miss in Burton Clark's book. Also in this context a time perspective is necessary as the economic role of education has undergone fundamental changes over time.

Before I put forward by arguments, I will give a short theoretical background. Since 1970 there have been radical changes of the world including structural and cyclical crises in the economy within which a slow growth in production, a chronically high level of unemployment, marked inflation and a low rate of investment propensity have been important factors. It might be stated that several of the conditions which reinforced each other during the 1950s and the 1960s, thus promoting growth, are now in opposition to each other, impeding continued growth. One example is the changes that have taken place in the area of employment. The tertiary sector has continued to grow at the same rate as earlier, while the secondary sector has declined drastically. In combination with the marked difference in the productivity development of the two sectors, this has created severe structural tensions in the economy, among other things in the labour market (job opportunities, relative wages) for those with higher education.

This turbulence in the economy and in the labour market has brought to the fore criticisms and reevaluations of established economic theories. One such reevaluation concerns the role of education in the economy, which can be said to reflect a change in society's appraisal of education. Especially in recent years several alternatives to the theory of human capital have emerged. A common denominator of these theories (signalling, screening, sorting) is that education is believed to function primarily as an instrument of identification or screening device, giving information to employers concerning the capacity and personal qualifications of individuals, while the productivity-augmenting effect of education is considered to be less important. Thus, the firms are primarily interested in the fact that the educational system ranks those seeking employment according to their inherent capacity. It is of no great importance whether the prospective employees have a lower or higher secondary shool examinations or academic examinations.(2)

I believe it is possible to identify three main reasons why signalling, screening and sorting models have been developed and why they have attracted so much attention during later years.

Firstly; one important new assumption, common to these theories, is imperfect information. According to the filtering/screening models the individual relays information about his own capacity to the firms by means of his choice of education, he himself possessing complete information about his own capacity from the start, while the firms do not possess this information. In the sorting models information about the individual's capacity is generated continually by means of education; an accumulation of information which will eventually lead to the individual being allocated in an optimum way. These latter models are based on the assumption that the individual does not possess complete information about his own capacity. Within economic theory in general, the same tendency to formulate new theories, based on the assumption of insufficient information instead of perfect information, can be found. This of course reflects the fact that the old theories no longer supply acceptable explanations of the current economic situation.

Secondly; it can be argued that changes have taken place in the employment structure during the postwar period (the rise in the number of office employees, the increased segmentation on the labour market), which have resulted in firms attaching less and less importance to the cognitive function of formal education and instead giving priority to its function as creating and transmitting information about the individual. The actual learning of skills is instead increasingly being aquired within the firm.

Thirdly; when the educational sector in general is subjected to an economic squeeze, it is convenient for politicians to resort to these theories for arguments. It is another story that they quite often misunderstand them.

The notion that education has an important sorting function for the firms, has largely been neglected in empirical work so far. However I believe an extensive part of the observed changes in the demand for postcompulsory schooling during the 20th century can be traced back to this specific economic role of education. Demographic factors are important in this context. Due to the fact that education has an important sorting function, the size of the age cohort, to which an individual belongs, will determine his choice of the level of education. Thus; an individual who belongs to a large cohort will attempt to improve his relative position vis a vis others by demanding more education; this is done in order to gain access to the few, relative to the cohort's size, favourable employment opportunities. Periods when the number of youth increases substantially will thus be characterized by an "inflation" in education. During periods when the youth cohorts decrease, the labour market conditions are favourable and this will result in a decline in the demand for higher education.

There is also strong empirical evidence to indicate that the size of age cohorts has influenced the demand for higher education to a high degree; not only in absolute terms, but also measured in terms of enrollment rates. This has been shown for Sweden 1923-1975 by Rolf Ohlsson(3) and for England 1945-1980 by Rolf Ohlsson and John Ermisch.(4) The American experience seems to be the same.(5)

I have argued earlier that size is an important underlying factor, when the organization of higher education and research is analysed.

Thus, it is obvious that the substantial changes in the demand for higher education - caused by the demographic factors - have enforced far-reaching changes in the organization of higher education. This is a powerful dynamic force, which I totally miss in Burton Clark's book and which ought to have been considered. I am fully aware that further research is needed to clarify more in detail the relationships that exist.

The future

It may seem inappropriate for an economic historian to predict what will happen in the future. But it is a well-known fact that the number of births have declined drastically in most West European countries and in the US during the period 1964-1983. This means that the number of youth going to universitites will decline gradually during the next 20 years, due to "pure" demographic reasons. The Swedish case can be cited as an example. Due to the fertility decline the age cohort 20 years will be almost 30% smaller in the year 2003 than in 1986. However, the number of youth going to universities will most probably decline even more. As I have argued above, an individual who belongs to a small cohort, doesn't have to improve his relative position vis a vis others by demanding a high education, i.e. enrollment rates are quite low for a small age cohort. If we assume that we have the same relationship between enrollment rates and the size of the age cohort in 2003, as the one which prevailed during the period 1945-1975, enrollment rates will be about 10% lower in 2003 than in 1986. Furthermore, it can be argued that structural changes in the economy will result in firms attaching less and less importance to the cognitive function of formal education, while instead the actual learning of skills is increasingly being taught within the firm. As a consequence the firms will try to recruit at younger and younger ages, while at the same time the universities will struggle for their existence as their hands in many ways are tied (low wages, insecure employment opportunities). I believe that this struggle is bound to influence the higher education system and the underlying "academic forces of production" in an almost revolutionary way in the future.

Summary

To summarize; to be able to grasp the common and basic elements in the higher education system, I believe an economic historical approach would be fruitful. There are, of course, economists and economic historians, who have analysed several important aspects of higher education and research. But to my knowledge, no economic historical study exists, where the central issue has been the changing organizational structure of the system. Such a study would, without doubt, give a useful complement to Burton Clark's somewhat static analysis and would also clarify more in detail some of the dynamic processes within the higher education system.

NOTES

1) Chandler, A. (1977) The Visible Hand. The Managerial Revolution in American Business. Cambridge, Belknap.

2) A more extensive presentation can be found in R Ohlsson: "The Labour Market and Structural Changes in the Economy - Higher Education as a Sorting Device?". Swedish Research on Higher Edu-

cation, 1984:7, National Board of Universities and Colleges R & D Unit, Stockholm, 1984.

3) R Ohlsson: Högre utbildning och demografisk förändring (Higher Education and Demographic Change). Lund, Novapress, 1986.

4) Forthcoming.

5) M Wachter and M Wascher: "Levelling the Peaks and Troughs in the Demographic Cycle: an Application to School Enrollment Rates". Reviews of Economics and Statistics, 1984.

Rune Premfors:

ARE SOME DISCIPLINES MORE RELEVANT THAN OTHERS?

Introduction

My concern in this paper is how to best further the development of higher education and research as a field of study. In particular, I will focus on the role and importance of various "disciplinary perspectives".

In simple terms, one may imagine three basic positions or strategies with respect to the role of disciplines:

(a) Disciplines pursue their research in an "isolated" fashion, utilizing higher education and research as one among many imaginable empirical illustrations. Knowledge cumulates as bits and pieces from the efforts of traditional disciplines.

(b) Disciplinary perspectives are integrated in a way which justifies the charachterization of the study of higher education and research as a new discipline. Knowledge cumulates largely through efforts within this new discipline.

(c) Processes (a) and (b) occur - albeit to a limited extent - simultaneously. A "core" of integrated and interdisciplinary efforts is surrounded by a larger "periphery" of single-discipline contributions. Knowledge grows through both types of process - but also through the interaction between "core" and "periphery". In particular we would expect traffic from the latter into the former.

Below I will discuss these three "basic positions" in both a descriptive and a normative or strategic perspective. I will argue that position (c) is both a reasonably good characterization of current trends and a defensible strategic position for the future.

A descriptive perspective: Two major traditions

Again in simplified terms, it could be argued that the field of higher education and research is dominated by two major traditions. I will label them "science studies" and "higher education studies" respectively. To be sure, borders are fuzzy and there is much, and probably increasing, overlap.

Science studies (science of science, research on research) is the oldest and most encompassing tradition. I will not try to trace its roots and development; others have already done it better than I could (cf e.g. Bärmark, ed., 1984). Its focus is scientific processes and the scientists - in whatever institutional setting they appear. Ini-

tially the field was primarily concerned with philosophical and historical studies, often with a biographical bent. Increasingly, however, the tradition has become dominated by a sociology of knowledge perspective, but with an important input of primarily philosophical and historical perspectives. Efforts have also been made to develop a distinctive theoretical base (a theory of science; cf Bärmark, ed., 1984, and Aant Elzinga's contribution to this report) as a "core" and with various social science and humanist disciplines as empirically oriented parts of a "periphery". In the Swedish context, the "history of ideas and science" is partcularly important (cf Sven-Eric Liedman's contribution above) among bordering disciplines.

Higher education studies represent a more recent and less encompassing tradition. Its focus is on the social institution of higher education (including its research activities) and its societal role. It largely grew out of "the 1960s" i.e. in particular the outbursts of student unrest and the growth of mass higher education institutions. Increasingly the "core" of the tradition is made up of studies of higher education - systems and, in particular, institutions - as an organizational phenomenon. But important research is also concerned with "input" and "output" aspects of higher education, such as social recruitment patterns and the role of higher education in elite formation and in the labor market generally. In terms of disciplines, the sociology of organizations - or, better perhaps, organization studies in general - dominate the "core", assisted by political science, management studies and pedagogy/education. But other subdisciplines of sociology - social stratification, sociology of knowledge etc - history (including, again, in Sweden the "history of ideas and science") and economics are also involved.

One may dispute details in this thumbnail sketch of the two major traditions, but I am confident that a study of their relevant "invisible colleges" would confirm the general argument: in view of the affinity of the research foci of the two traditions, there has been remarkably little interaction among them.

More important in the context of this paper, however, is that both traditions may largely be characterized in terms of position (c) above. Most contributions may be classified as single-discipline efforts; if this was all there was, we would in descriptive terms be in position (a), and very far from prosition (b). However, this single-discipline dominance (in quantitative terms) is in both traditions complemented by a "core" of efforts which contain significant elements of interdisciplinarity. In science studies this "core" centers on a theory of science/sociology of knowledge perspective; in higher education studies it consists rather of an organizational perspective.

A strategic perspective

To me position (c) as outlined above not only represents a reasonably good description of the current trends within higher education and research (as a field of study); its blend of a few interdisciplinary and many single-discipline efforts also represents the best strategy for the future. The challenge may be said to consist in identifying the nature of these trends and in formulating measures to strengthen them. In support of this normative stance I will first argue against position (b) - which entailed efforts to create a new "discipline" - as well as position (a) - which implied single-discipline perspectives

only, before I elaborate my vision of the future in terms of position (c).

What are the chief arguments against turning the study of higher education and research into a new, integrated discipline? I think such a strategy is both unrealistic (in most national contexts) and generally undesirable. Only in the United States does there seem to be sufficient "social demand" and resources for a significant number of specialized educational and research units in the field of higher education and research. Most of these are either departments of higher education, chiefly oriented toward the professional training of administrators, or research institutes leading a rather uncertain life in a long-term perspective. It may be too rash a judgment, but many would argue that the best research in the field of higher education and research in the U.S. is <u>not</u> performed within such permanent or semi-permanent settings. Instead it emanates from researchers or research groups linked with research environments of a broader kind - traditional departments or institutes encompassing more than the study of higher education and research. Specialized departments of education do not seem to be able to attract the best talent among those who pursue research in the field (there are of course many individual exceptions). Relatively few social scientists and humanists seem to be willing to view the field as a life-long commitment, at least in the sense of giving up their career within the broader framework of one of the traditional disciplines; they obviously want to be historians, sociologists, political scientists etc first, and only then students of higher education and research.

Whether this is due to the lack of a sufficient number of career opportunities in the latter field or to a conviction that the traditional disciplines are "more important" in the context of "scientific advancement" is of course impossible to say. Which set of motives dominate is of importance from a strategic perspective: in the former case we have to do with a "vicious circle" which may be broken - a sufficient number of career opportunities would attract sufficient talent; in the latter case we have to do with a more rigid "structural" feature - higher education and research as a field of study seems largely bound to be considered inferior to the established disciplines.

In the Swedish context, efforts to "discipline" higher education and research as a separate field of study seem even more unrealistic and undesirable. Largely this is simply due to the much smaller size of the existing research community and - with realistic assumptions - the possible "higher education and research community". Today a very small number of researchers are wholly oriented toward the field; even fewer intend to stay there so long as to consider it a life-time career.

In sum, then, the field of higher education and research will not realistically develop into a full-fledged discipline. With sufficient resources poured into the field it may take on a number of superficial features of one - such as in the United States where a large number of departments of higher education exist - but this would not imply the creation of a vivid field of research full of exceptional talent.

Do we, then, have to accept strategy (a) - knowledge growth through bits and pieces produced within the context of the traditional disciplines - as the only realistic strategy? It would be unfortunate. None of the traditional disciplines could persuasively argue (despite sometimes heroic efforts; cf Lennart Svensson's claims in his contribution to this report on behalf of education) that they cover most of higher

education and research as a societal institution. Thus different disciplinary perspectives are needed in order to provide a broad "coverage". But as with other major social institutions, the traditional disciplines leave us with blanks and an all too limited understanding of significant relationships. Multidisciplinarity is not enough; interdisciplinary efforts are needed. It may be argued that this "integrating" effort should be the responsibility of policy-makers and other stakeholders as consumers of research knowledge. To an important extent I consider this a valid point, but far from entirely. It would be entirely valid if the blanks and the missing relationships were only relevant to higher education and research as a field of practice. However, I am prepared to argue that they are equally problematic for the field as an area of "basic research".

Finally, I will try to elaborate position (c) as a strategy for the future. In the Introduction it was outlined as a position /strategy where there was a "core" of interdisciplinary efforts, surrounded by a "periphery" (perhaps quantitatively dominating) of single-discipline contributions. My attempt at a descriptive account of the current situation within the field of study argued that its two major traditions could in fact each be described in such terms. In sketching a strategy adhering to position (c) below I will, first, argue that a chief aim of such a strategy should be to integrate the "cores" of the two traditions - to try to marry the knowledge focus of science studies with the organizational focus of higher education studies. Second, and in a separate, concluding section of the paper, I will spell out which disciplines are in my view more relevant than others.

To characterize the "core" of science studies as knowledge-focused and that of higher education studies as organization-focused is of course a very crude statement. Many a research in the former tradition has centered on organizational variables, including those present in higher education settings (for an excellent illustration, see Bulmer 1984). And within the tradition of higher education studies an important trend during the last years has been the increasing attention paid to the research function of institutions (cf e.g. Wittrock & Elzinga, eds, 1985); also several important contributions have within the context of an organizational perspective emphasized the implications of the "knowledge-intensive" character of higher education.

A prominent example in the latter vein is Burton Clark's <u>The Higher Education System</u> (1983). Clark shows convincingly how fundamental features of higher education organizations - from basic units to national systems - are determined by the logic of their basic task structure. These organizations are unique in many respects and differ significantly from "industrial organizations, governmental bureaus, and the many agencies that dot the nonprofit sector". At the same time, however - and this, one may add, is a good supplementary reason for studying them in depth - "other milieus are becoming similar to higher education as they base themselves on knowledge, science, and profession" (p. 11).

It is of course no coincidence, then, that the first chapter of Clark's <u>magnum opus</u> is entitled "Knowledge". It is also not surprising, considering the admirable scope of the book that there is little room for specifics and nuances with respect to various types and forms of knowledge in higher education, and the detailed organizational implications of such variations. The book is rich enough, but it may have profited further by drawing on important current work in this area (e.g. that of Richard Whitley; cf Whitley 1984 and earlier works; also Becher & Kogan 1980). As it now stands, Clark's analysis

largely rests upon a rather simplified notion of traditional disciplines and university departments.

On the whole, however, The Higher Education System represents an important effort to integrate knowledge and organization perspectives in the study of higher education and research. An important further example of such efforts is a project which has recently been completed by a group of researchers ("FORSTYR-gruppen") at the School of Business Studies in Copenhagen. A wealth of insights is contained in three books by the group members (Agersnap et al, 1983; Fivelsdal, ed., 1985; and Foss Hansen 1985). In this context, two features of the project should be noted. First, a conceptual framework is developed dealing with basic "forms of coordination" ("styrformer"). In contrast with most earlier similar attempts in the field of higher education and research (including Clark's and my own; cf Premfors 1986) it specifically includes "knowledge" (also termed "dialogue") as a specific and in the context highly relevant form of coordination (the concept is partly inspired by Habermas; cf Beck Jørgensen & Larsen in Agersnap et al, 1985; also Foss Hansen 1985).

Equally important, however, the research did not stop at innovative conceptualization. The analytic framework was applied, modified and tested in a number of careful case studies (four university departments and a sectorial research institute). The empirical analysis strongly supports earlier observations concerning higher education organizations as extremely complex, "loosely-coupled" systems. But even more significantly, it illustrates the great variety among such organizations and provides a wealth of ideas with respect to the basis of such variation. Just to mention one such basis: the interrelationship between the teaching function and the research function of a department. A department may well, for example, conduct highly specialized, "internally determined" research, and at the same time engage in teaching activities which are quite elementary and wholly determined by "external forces" (a modern language department is a case in point).

Here I have not done justice to either of these two impressive research efforts. My purpose has rather been to suggest briefly - by citing the best examples I know - the direction in which I think the "core" of higher education and research studies should travel in the future: toward a synthesis of the knowledge-focused perspectives of science studies and the organization-oriented approaches of higher education studies.

Conclusions

My preference, then, is for strategy (c) as outlined in the Introduction above. The study of higher education and research should not strive for the status of a new discipline, nor should it proceed by the "equal misery"-principle, spreading available funds to all social science and humanist disciplines. There has developed a "core" of integrated and interdisciplinary efforts surrounded by a "periphery" of single-discipline contributions, and this set of current trends should be consciously strenthened.

The "core" of the field (at its present stage of development) should primarily consist of efforts which seek to integrate the perspectives and results of the science studies and the higher education studies traditions respectively. To accomplish this requires a determined

interdisciplinary effort. In terms of specific disciplines and their subspecialties such an effort should at least include sociology (both the sociology of knowledge and that of organizations), history (in particular the history of ideas and science) and education. In Sweden, the theory of science evidently also belongs to the set of key contributors within the "core" of the field.

In conclusion, then, some disciplines are in my view more relevant than others in furthering basic knowledge of higher education and research. Three important qualifiers should, however, be added. First, to some significant extent the "core" lives off the "periphery"; both are important although the "core" has been singled out for preferential treatment. And, equally important, the presence/absence of competent or even excellent researchers is a self-evident constraint, as in all efforts to further basic knowledge. In such contexts, no research may even be preferable to bad research. To put it differently: if high-quality research cannot be promoted within the favoured disciplines, then strategy (a) is preferable. Finally, the strategy advocated here is heavily context-bound; I consider it the most useful one for the near future and, particularly, in the Swedish setting. Again, in other and different contexts, strategy (a) - relying chiefly on the internal dynamics of the entire set of social science and humanistic disciplines - may be preferable.

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Burton R. Clark:

EXTENDING THE HIGHER EDUCATION SYSTEM(1)

System was written between 1978 and 1982. What I knew at the time came principally from three sources: my own research, which was largely U.S.-based but included a case study of the Italian national system of higher education; the research of a small group of young scholars and visiting professors studying different national systems, from Sweden to Japan, that I assembled year-to-year between 1974 and 1980 in a Yale research group; and the research and reflection of a growing number of experts in other countries. In the early 1970s, scholars who had offered major integrated cross-national statements about higher education were so few in number that one could virtually name Eric Ashby and Joseph Ben-David and stop there. By the early 1980s, the ranks of significant contributors had grown considerably, even if their work centered mainly on their home countries. Among others, one could—in a comparative fashion—learn about Japan from Ikuo Amano, Kazuyuki Kitamura, and Morikazu Ushiogi; the United Kingdom from Tony Becher, Maurice Kogan, and Harold Perkin; Sweden from Rune Premfors and Bertil Östergren; the Federal Republic of Germany from Dietrich Goldschmidt and Ulrich Teichler; and even France from the occasional observer whose "factual" statements elicited some agreement from other French scholars.(2) Starting from a very small base a decade earlier, a rapidly-growing but fragmented literature was in place in the early 1980s.

My intention in System was to establish an integrated framework for cross-national comparison, some basic categories that would organize and link diverse empirical observations. The effort was driven by a particular set of analytical biases: to understand universities and colleges holistically as complex organizations; to move organizational research to system levels; and, since I had long been annoyed by the tendency in education and in much organizational analysis to avoid questions of power and control, to broaden my analysis in the direction of political and political-economy modes of thought. The realities of comparing American or Japanese higher education to Italian or French or Swedish higher education virtually forced me in this latter direction, in order to conceptualize radically different forms of national control and organization. As the manuscript progressed, I found myself swimming in the broad currents of "politics and markets," even clinging to such slippery rocks as "the hidden hand," "muddling through," and "market failure."

Although thereby broadened to the point where some have seen the effort as interdisciplinary, a sociological-organizational bias remained. The intent was to concentrate on the higher education system itself and to do so by means of organizational analysis. I would be an internalist who took seriously the professionals and bureaucrats who manned this sector, an analyst who looked out upon the world from the internal sites of work and authority. There was no intention of trying to cover the waterfront of empirical realities, let alone the gamut of perspectives. The most important limitation came from the blanket decision not to do "higher education and society." I would not devote various chapters to how higher education related to the economy, the social-class system, the family, the religious order, and the other main institutions of society. That 1,000-page omnibus treatment would have to be written by someone else, an externalist whose work would

probably suffer from a lack of understanding of the internal workings of the system itself, especially its differentiation by discipline and type of institution.

The bias persists. In any effort, now or later, to update, extend, or otherwise revise the framework evident in System, I want to remain with my initial approach, adjusting it at the margins. The analysis should be partial rather than all-inclusive, specialized rather than generalized. But as evident in the papers assembled for this seminar, other perspectives are increasingly demonstrating their value. Much good work is underway on a wide range of topics. I would like to refer first to the perspectives and topics that I am best able to relate to my own framework and then to touch only briefly on the ones where the connecting bridges are weaker.

Topical extensions

Rich indeed in recent years is the work of international colleagues in a complex of related topics identified by the three terms "discipline," "profession," and "science." Compared with just a few years ago, there is much that can now be highlighted about disciplinary, professional, and scientific phenomena that helps us understand the dynamics of higher education. In my own country, the farflung literature on higher education remains almost naively student-centered, and secondarily institution-centered. While concentrating on student life and the possibilities of improving the general education of undergraduates, it avoids analysis of the thrusts of the disciplines, the long-standing habits of the academic profession, and the imperatives of science. The discipline is so much a primary unit of modern higher education, the profession is so central to effective performance, and science is so basic in academic organization that the increasingly promising work of the scholars who center on these topics needs to flow more readily into the literature on higher education systems.

The discipline

System attempted to highlight in the chapter on work organization the division of academic activities by discipline as well as by institution, referring to these two crosscutting lines of affiliation as the master matrix of the system as a whole. I also discussed in a fragmented way the cultures of disciplines, quoting a mathematician here and a philosopher there, in an effort to give some life to the idea of disciplinary cultures. These sections could now be greatly improved, first of all because we now have the recent work of Tony Becher as a window on the internal lives of the disciplines. When Becher completes the book he is writing on disciplinary differences, we will all be much better informed. In the meantime we learn from his occasional papers about cultural differences in and across disciplines. We are made to realize more clearly than before that there is a strong epistemological determination of academic subcultures. In the paper at hand, we are led into the various discourses of the disciplines, and thereby into an understanding that we can use broad linguistic analysis as a way of understanding disciplines. Here we learn in fascinating detail about how different academic life really is, in the very bones of expression, in a tribe that is secure in its weapons and boundaries (physicists), compared to a tribe whose anxieties recapitulate a fair share of the angst of the modern world (sociologists); of how different it is to work embedded in strong theory (physics) from

laboring in a field that is largely non-theoretical (history). We are offered one promising insight after another: that the language used to evaluate research matches the epistemological properties of the subject, with implications that the scientific ethos splits into as many tongues as there are disciplines and significant specialties; that the publishing habits of disciplines indicate one is "a large-scale academic industry with a rapid and highly international communication system" while another is a much more limited collectivity in both organizational scope and geographic reach; and that disciplines display fundamental differences in the type of evidence and procedure they use for proof of the truth.

In our efforts to grasp the business of higher education, there was never any excuse for not taking seriously the ways of the disciplines. Now, ignorance can no longer be pleaded. And think of the opening provided for dissertations!

The profession

System contained a few general comments about the academic profession. I noted that this profession was inherently more diverse and fragmented than others; and I speculated about its culture, emphasizing the pervasive influence of the norms of science. Having pursued the academic profession in research during the last three years, I am persuaded that it should be treated as a major topic. An international conference held in Bellagio in the summer of 1984 has led to a cross-national manuscript that offers informative analyses on the academic profession in the United Kingdom, the Federal Republic of Germany, France, and the United States, and four analytical essays on the general differences between European and American patterns, the impact of the disciplines on the profession (a Becher paper), and disciplinary and institutional differences in the American system.(3) This set of papers teaches us about substantive as well as reactive growth of the profession--growth driven by an expanding knowledge base as contrasted to growth that follows expansion in student enrollment. We learn about the differences between an academic profession that is on the national payroll and one characterized by extraterritoriality in relation to central government. We also learn about the differences created by continuous rank structures compared to discontinuous ones. This volume alone measurably extends the literature on the holistic organization of academic life that is fashioned by academics themselves.

On the American academic profession, Howard R. Bowen and Jack H. Schuster have recently published a volume in which they pull together much material, especially on compensation and conditions of work.(4) They express concern about possible future weaknesses in the pipeline of talent if some of the adverse conditions of employment faced by professors in the 1970s and early 1980s should continue. From a three-year domestic study, I will publish a volume in 1987 that emphasizes the breakup of the American professoriate in different worlds created by types of institutions as well as by diverse disciplines.(5) I juxtapose, at the extremes, the community college teacher and the professor in the leading research university. Their jobs differ radically. In the universities I compare, for example, the clinical professor of medicine whose daily round of work is in the cancer ward of the medical school hospital and the scholar in the classics department who is immersed for a life-time in Dante. For the most part, professional schools and letters and science departments in American universities are different kinds of places. Then, too, one-third of American

academics are now part-time, and part-time status is very different from full-time employment. The emphasis on different worlds articulates well with Becher's depiction of differences in the culture of the disciplines. What one must add in the American context is the great differentiation produced in the academic profession by the distribution of professors among ten or more sectors that offer different combinations of research and teaching, different clienteles, different connections to the labor force, and even different investments in knowledge.

Science

Whatever "the theory of science" is, its leading practitioners are clearly on to something. Aant Elzinga's thought-provoking paper makes this abundantly clear. There are so many valuable lines of thought here that I can only react to a few. Key for me is the emphasis on knowledge structure as a phenomenon separate from social structure. When Elzinga looks at the academic world, he apparently first sees not groups of people but bundles of knowledge. He is deeply interested in "the relative autonomy of the cognitive dimension," and in how theoretical and methodological problems become determinants of change; in how "epistemological resiliency and stability expresses itself, and what it signifies"; in grasping that "the cognitive shape of a discipline" is an independent variable in determining the attitudes of scientists; and in showing that epistemic drift is equally if not more important than academic drift.

At the same time, Elzinga turns to social structure. When he attends to carrier groups, people are brought back in. We plunge into a world of academic interests. In the example of the Swedish Coniferous Forest Ecology Project, the SWECON Project, Elzinga tells of 80 researchers from various disciplines, headed by a Steering Group, going through "several distinct phases of crisis and reorganization during a ten year period." One can virtually smell the sweat of debate and interaction, with various sub-groups pressing home their vision of how to proceed. In the probing discussion of epistemic drift we are told of a shift from a traditional system of control in knowledge structures to externally based regulatives and controls. "The latter crystallize around sectional functions of knowledge utilization (housing, defence, development aid, health care, social problems, etc.) as defined by the Ministerial structure within the state apparatus..." (emphasis added); or around the preferences of definable groups of people in social and political movements; or, thirdly, around the "commercialization" interests of business and industry. In the revealing material on France that Elzinga draws from Regis Debray, we follow "a shift of focal power" from academics to publishing houses to media intellectuals that leaves, at the bottom, "an abject educational system."

In the illuminating concept of "hybrid communities" we directly confront social structures. We are led to focus on "the emergence of special groups of people who function as brokers or intermediaries between the respective domains" of science and policy-making. They connect "two spheres of interest." As Elzinga points out, it would be interesting indeed to compare how these hybrids function in different national and cultural settings: "one point of comparison would be to see who populated the hybrid communities, because in their composition one may get some reflection of the various stakeholder interests."

And so for the emergence of new disciplines, yet another significant topic highlighted in Elzinga's paper. Here we learn about the struggle

of the new theorists of science to liberate themselves in Sweden from the mother discipline, theoretical philosophy. With representatives of the old discipline wanting "to influence the definition of the new subject and populate 'positions' in the field," the emerging group has to struggle to establish its own nationhood. In the emergence of nursing care research as a specialty in Sweden, we face ultimately the "question of who is doing the defining: nurses, physical therapists or bureaucrats and administrators. Each and everyone has a stake in the new subject."

What is so fascinating about new disciplines (or interdisciplinary subjects) is the rate at which the modern academic world produces them: for example, in the social sciences, environmental research, peace and conflict research, women's studies, urban studies, ethnic studies. Helpful here is an analysis by Walter Metzger, an historian of higher education, of how "substantive growth" occurred in the American academic profession in the nineteenth century by means of a set of processes that are different from the "reactive growth" of faculty attendant on student expansion.(6) Metzger empirically fleshed out four processes. Parturition is the birth of new fields out of a mother discipline. Program affiliation is the incorporation into academia of training in such established fields as law and medicine. Dignification—the most fascinating process in the "open" American setting—is the incorporation and raising to respectability of fields that formerly were beneath the dignity of higher education: the arts of the home became home economics and then domestic science; the ways of the parade ground and the battlefield became military science. Dispersion is the process of subject imperialism. History expands to cover all of time and all of the world. There is now an economics of everything. Nothing is safe from the eyes of roving sociologists. These processes clearly feed one another, with subject dispersion a leading cause of parturition: new foci of interest emerge in a discipline and then break away as separate disciplines.

I have selected from Elzinga's paper the discussions that make him a student of academic organization. I find his paper most congenial to the perspective that drove System when he aims directly at the groups that carry knowledge. But his primary emphasis on the structure of knowledge is also one that would measurably extend System. I started and ended the book by emphasizing that higher education centered on knowledge. But I did not have the wit nor the competence to discuss the structure of knowledge itself, to seek out the ways of the cognitive structure. With another year of homework, reading Aant Elzinga, Björn Wittrock, Stuart Blume, John Irvine and Ben Martin, Arie Rip, Michael Gibbon, Andrew Jamison, Michael Mulkay, and others, I hope to have the wit and some of the necessary competence to treat the theory of science seriously. But I will still put social organization first, placing front and center the social groups that labor to produce and distribute advanced knowledge. The subtitle of the book—academic organization in cross-national perspective—is not one that I would change.

To extend the metaphor offered by Elzinga of the blind observers sensing the elephant: when he and I confront the elephant of higher education, we are not so far apart as the tail and the trunk, a piece of string and a large snake. Elzinga particularly listens well to the noises that the elephant makes, joining Tony Becher as an observer of discourse, but he also cannot help noticing how the elephant moves. I fixate on how the elephant walks, but then I also cannot help noticing the sounds he makes as he lumbers along. Circling the beast, luckily with eyes open and ears operative, we converge in an understanding

that what we have before us is a large, ungainly, generally slow-moving animal who, most of the time, snorts and moves straight ahead, who can perform marvelous tricks if given suitable incentives or left to his own devices, and who becomes balky when pushed or ridden too hard. This is some improvement over mistaking the elephant for a tree!

Not far away from "the theory of science," apparently, is the perspective of "the history of ideas and science" that is ably exhibited in Sven-Eric Liedman's paper "Integration and Higher Education." Here we confront three themes: the importance of viewing higher education from an external perspective, a subject to which I will return; the nature of specialization; and the possibilities of integrating academic systems. The tension between specialization and generalization, fragmentation and integration, is central to my own thinking, first as a sociologist who finds merit in Durkheim's great concern about the division of labor in society and its disintegrative effects, and then as a student of organizations fascinated with the forces that cause modern complex organizations to fall apart and the strength of the opposing mechanisms that bureaucrats and professionals create to hold things together.

Liedman's discussion of specialization is helpful. He makes a distinction between fields of work and fields of competence, points out that in a true specialty the two coincide, and notes that there is often an inner tension between the two. Particularly stimulating is his discussion of the possibilities of academic integration. He notes that "the circumstances of integration are not the same everywhere," and points to differences over time and space: Berlin in the early nineteenth century, Berkeley in the late twentieth; the Swedish university system today compared to the German or Japanese or American. Each national tradition has its own logic: integration in Sweden is aided by a tradition of public communication of scientific results, whereas that tradition is "definitely weaker in most other countries." It is then possible to portray the Swedish university intellectual as a "civil servant close to the people," one who has the double task of integrating the scattered branches of science as well as deepening specialist knowledge, and to adopt a normative posture that calls for a revitalization of this type of intellectual.

Early in his paper, Liedman also argues for a "real reconstruction" of general education that would start from "scientific integration." We need not only specialties. In order to move in this direction "we have to give up the dogma that real research and advanced studies must be specialized," and to realize that traditional philosophical problems still have the power to unite different specialties.

On all such possibilities of integration--within the university on one level, within the higher education system at large on a second, between that system and other sectors of society on yet a third--national differences matter enormously. In my chapter on integration in System, I pointed to some of these differences in a triangle of coordination that juxtaposed state, professional, and markets forms of coordination. National systems are quite variously located among the coordinates of these possibilities. For example, the large countries that have major private sectors of higher education (Japan, Brazil, the United States) stray far from the unifying potentials of state and professional mandates and drift toward forms of linkages in which patterns are worked out largely unconsciously and indirectly in thousands of transactions in consumer, labor, and institutional markets.

In my recent work on the academic profession, where the contradictions of fragmentation and integration were taken as central, I was struck more than ever by the vast differences occasioned in national systems of higher education by size alone. As of the early 1980s, academics in Sweden numbered about 11,000 and were located in about 40 institutions; in the Federal Republic of Germany, they were in the range of 125,000, in 250 institutions; in the United States, the official counts of academic personnel swell to 700,000-800,000, in over 3,000 institutions. The United States has virtually as many faculty members as Britain has students! With all Swedish academics totaling about 11,000, we can observe that many individual disciplines in the United States are larger. U.S. academic mathematicians and statisticians number over 30,000, academic chemists over 18,000. Psychology contains about 25,000 professors, economics, 15,000. English has 50,000 to 60,000. Mind-boggling! The possibilities of integrating academics in a profession are qualitatively different from the case of Sweden. Scale alone dictates that whatever integration obtains, it is not likely to be found in commonalities of relationship and interaction. When we add the diversity of types of institutions in the American case, the forces of fragmentation take a quantum jump.

What, then, remains of the possibilities of integration in the international centers of learning that are many times the scale of the Swedish system? What remains is an overlap of memberships and commitments (and cognitive structures!) among disciplinarians that connect them in partial ways. Most academics belong to a subspecialty within a discipline while they also belong to the discipline as a whole. Some belong to a discipline and to a multidisciplinary unit: a professional school (medicine or education) that incorporates people from various disciplines; an area studies program (African or Latin American studies); or a problem-centered unit (urban or environmental studies). Some disciplinarians are active in interdisciplinary associations (e.g., a social science research council), and in omnibus faculty associations (e.g., the American Association of University Professors). Diana Crane, a sociologist of science, has acutely observed that the social system of science is an appropriate model for how cultural integration may co-exist with cultural diversity in a highly differentiated society: "contemporary science comprises hundreds of distinct specialties, but each specialty has connections, both intellectual and social, with other specialties/.../cultural integration occurs because of overlapping memberships among cultural communities that lead to the dissemination of ideas and values..." What we find are "interlocking cultural communities."(7)

This conception of overlapping cultures and social systems has been given vivid imagery by Donald T. Campbell: each narrow academic specialty may be seen as a fish scale overlapping other scales. A comprehensive social science, or any other large domain of knowledge, is then "a continuous texture of narrow specialties." Integration is "a collective product, not embodied within any one scholar. It is achieved through the fact that the multiple narrow specialties overlap and that through this overlap a collective communication, a collective competence and breadth, is achieved."(8) We might think of overlap as a hidden hand of integration, with collective products and relationships issuing from thousands of actions in semiautonomous parts.

The implications for reform are profound. Campbell insists that efforts to connect fields by training scholars to master two or more disciplines are largely destined to fail. Whenever we deliberately set out to create generalists who will again freely range over a large territory of knowledge, we create thinness that soon falls into dis-

repute. Instead, the realistic way to proceed, one that fits the ways of the modern academic profession, is to make those organizational inventions that will encourage specialization in the interdisciplinary areas. The interdisciplinarian must "remain as narrow, as specialized, as any other scholar." As we master the logic of this perspective, the slogan for reform becomes "collective comprehensiveness through overlapping patterns of unique narrownesses."(9) Overlap of components is the central idea. In Michael Polanyi's terms, there are "chains of overlapping neighborhoods."(10)

In short: I share Liedman's great interest in academic and scientific integration. But we are well beyond the time in the large national systems when deliberate effort can effectively promote a return of the academic generalist. A little can be done: some scholarly rewards are still apportioned to some academicians who range across disciplines. Those rewards can be incrementally enhanced. But specialization is at the heart of the disciplinary imperative. It is better then to seek the ways in which integration is built into that logic or can adaptively utilize its force. Even as differentiation runs wild, academics can still be chained together in sequences of linked specialties. Scholars also follow problems across old disciplinary boundaries. New perspectives occasionally regroup fields in an integrative fashion. Given suitable organizational footing, as in the leading liberal arts colleges in the United States, academics can even manage to give a liberal or general education to some students some of the time. There is much that we can learn if we follow the lead of Crane, Campbell, and Polanyi on latent forms of integration in highly differentiated domains. At least this approach is now a promising way to approach the ultimate sociological problem: how social order is maintained in sectors and societies evermore fragmented by the advancing division of labor.

Thorsten Nybom's paper on "Research Organisation and Research Policy in Sweden after the Second World War" continues the focus on discipline, profession, and science. He presents, in effect, a case study of mandated science versus disciplinary science, and mandated academia in conflict with the traditional ways of the university and its professoriate. As an outsider to the details of the Swedish case, I am struck by Nybom's point that the Swedish system of higher education "has been extremely open for external penetration." The penetration involves, and has been driven by, "massive bureaucratisation," the latter reflecting in Sweden "a general principal for societal decision-making and power-distribution" that can play upon any sphere of modern society but which has found especially vulnerable the subsystems involved in "knowledge production in general and research in particular." Such vulnerability is surely a basic matter. It involves the role of governmental commissions that have followed one after another, the corporatist relation that turns external groups into internal ones, the nature of research council organization, and especially the sectoral line of "intrusion" that seems to have been prominent in Sweden in the last decade. One confronts an "interventionalist society" possessed by a super-ideology that places research and higher education at the center of political planning and insists that these activities should be evermore integrated with other social and economic activities. Practical application comes to the fore. Short-term needs, as defined by others, weigh more heavily. The scientific ethos is pressured to take up a more utilitarian focus. Doctrines of "fair share" loom larger.

In a cross-national vein, Nybom points out that these processes have not been identical everywhere in western society, that their strength

and effects will "depend greatly upon the construction and formative characteristics of the existing system of research, not least the position and status of the traditional academia." He notes specifically that Sweden has not heavily used "two of the most common and successful means" of organizing research, namely, the university-based research institute and the independent but highly integrated graduate school. Absent these tools, research is all the more likely to drift out of the university, leaving it a secondary role. The existence and strength of such tools is a fundamental matter that should be pursued cross-nationally. The trajectory of my own research points in this direction. I will initiate in the summer of 1987 a three-year comparative study of the changing relation between research organization and advanced training. Central in such inquiry is the historic nature of the graduate school and its counterparts. Also apparently central is the shift in governmental support of research from institutional and research council lines to sectoral lines, a shift that alters university research organization and, in turn, the nature of advanced training. In this fruitful area of research, many perspectives will be needed.

To conclude on discipline, profession, and science as congenial extensions of System: I agree with Rune Premfors' comments in his paper that "science studies" and "higher education studies" are two mainstreams of thought at the core of current interest that can be brought closer together. The first stream is particularly guided by the sociology of knowledge and the newer, more embracing theory of science. The tradition of higher education studies is fed particularly by broad organizational analysis that extends somewhat into history, political science, and political economy. Premfors' statements about core and peripheral fields of study need not be normative. They can be taken as descriptions of several traditions that have seized the center of the stage, and of other fields, only lightly staffed, that have yet to move from the background into the spotlights of attention and explanatory power. Just as the overlap of the two core traditions can be increased, so can the interaction between the stronger and weaker fields. Much is left to the type of will evident in the organizing of this seminar that seeks to bring additional fields into the discussion and to encourage them to develop an interest that is at once continuous, cumulative, and self-amplifying.

I can also note at this point that I am unable to react effectively to the point of view that Lennart Svensson seeks to develop in his paper on "Higher Education and Research in an Educational Perspective." I cannot grasp what a separate educational perspective would consist of. Instead I see many different types of analysts studying education from many perspectives, preeminently those of the major social sciences and various specialities within them. In the study of the creation of knowledge, of the refinement and perservation of knowledge, and of the transmission of knowledge, analysts will use diverse perspectives that cannot be fused into one. Hence, from my perspective on perspectives, I do not think it is possible to define "an educational perspective" into existence as a tool for analysis.

Historical development

System could have used a better sense of history: developmental analysis should be an important part of the conceptualization of academic organization. Here, Rolf Ohlsson's concerns are relevant: the "time perspective" somehow needs to be addressed. If System were written now, I would try in virtually all the chapters to attend more to the

historical flow of change and continuity, especially for the nineteenth and twentieth centuries. I am increasingly persuaded that Ohlsson is absolutely right when he writes "that neither the general nor the specific features of the present system /can/ be understood unless due emphasis is given to the historical development."

But limitations soon appear in the scholarly art of the possible. The knowledge base is still a thing of bits and pieces. While monographic work continues, integrative efforts by historians are needed, particularly to inform non-historians. One helpful global statement is an essay by Harold Perkin on the historical perspective.(11) In the international volume on the academic profession that is in press, each of the four country chapters, three written by historians, presents some historical development.(12) Konrad H. Jarausch has edited a useful volume of some sixteen historical papers that analyze the four processes of expansion, diversification, "social opening," and professionalization in England, Germany, Russia, and the United States in the late nineteenth and early twentieth centuries.(13) Alexandra Oleson and her associates have published or have in press three edited volumes on the development and organization of knowledge in America, from the colonial period until today.(14) But much more is needed. Harold Perkin currently has in mind the possibility of preparing a major book that would be simultaneously historical and comparative, a feat—one for which he is ideally suited—that would require command of a very large body of material. I look forward to the early 1990s as a time when we might have in hand a literature on the historical development of higher education in various countries that non-historians might more readily incorporate into their work.

But, even then, science studies and higher education studies will face limitations in attending to the historical dimension. One constraint is the personal competence of non-historians in mastering historical materials. A second limit is the practical matter of book manuscripts becoming tedious, even unpublishable, as they swell in size. A third item to keep in mind is the likelihood that in scholarly efforts to include everything, analytical sharpness will suffer. Thus, even when the literature contains integrating work that is both cross-national and historical, analysts of academic organization will be hard put to use it succinctly and effectively, lengthening manuscripts by no more than a fifth or a fourth while using the knowledge of historical development to sharpen analytical bite and to enhance explanatory power. One will need to concentrate broadly on genetic imprints, basic trends, and the trajectories of tradition and received structures.

"Society"

Various colleagues in this seminar have noted that System does not pay much attention to the world outside of higher education. As stressed in the book's introduction and remarked at the outset of this paper, such avoidance was deliberate. There was more than enough to do in concentrating on the interiors of national systems of higher education. As we dig inside the black box of higher education, we also avoid easy imputation of influence to "society" by finding out first how much the on-the-spot actors are driven by imperatives and constraints embedded in their own sub-system. Why reach for remote, ultimate causes when more immediate frames offer explanations that are direct, clear, and parsimonious? We ought to take seriously the point vividly made by Ralf Dahrendorf when he remarked that "certain areas of human activity have evolved their own action patterns: the world of science, or of painting. There is, in other words, such a thing as

sectoral hegemony."(15) Following this logic, analysts are well-positioned to figure out how the much-discussed autonomy of higher education is fashioned. We turn directly to academic culture, disciplinary cultures, and the ways of science as expressed in disciplinary science. We take professionalization and bureaucratization seriously, believing with good reason that when these two "iron cages" of the twentieth century fashion large organizations and larger sectors, powerful social actors are thereby produced. In short, we immerse ourselves in the organized ways of a set of strange modern tribes.

How, then, can observers handle analytically the interaction between those in the system and the rest of society. First, by differentiating the internal agents. Then we do not lump together the medical school and the humanities departments as if they automatically had the same relations to government and to the economy. Such disparate academic units are likely to have quite different external relations, even opposing ones. As mentioned before, it is quite essential in the American case to give up the simplistic language of "higher education" that confuses the community college with the research university: their so-called "functions" in society, their relations to outside groups, could hardly be more different.

Second, the internalist handles "society" by watching the internal agents and their sub-systems respond to external demands and trends. The shaping of response becomes central. How does the demand or trend flow into the system? How is it redefined, implemented, attenuated or otherwise shaped by the internal groups? Their interests, their traditions, their cognitive maps are taken as intervening variables between demand and response. Then, too, it is quite common for academic agents to steer "external demands" in the first place: e.g., when they establish entrance standards and publicly announce them, thereby shaping the perceptions and choices of tens of thousands of young people who are normally subsumed in the great "environmental" category called "consumer demand."

An internalist perspective centered on academic organization will come up short for those who want to study the relation of "higher education" to social stratification, or political development, or economic trends. But it beams analysis on the immediate settings of academic action. In an age when professionals and bureaucrats have shown their capacity to seize and command one domain of work after another, it pursues the tools of academic hegemony. It understands that subculturing and substructuring are powerful phenomena in modern societal sectors, not least in one based on arcane knowledge. And it looks out on the world from inside the system, as the incumbents do, to sort out how they see the outside world, how they respond to its demands, and how they use it as best they can according to their own definitions of the situation. There are good analytical grounds for stubborn pursuit of an internalist perspective.

At the same time, however, I marvel at the insight that can be obtained by others when they work an externalist perspective in a highly-focused way. Tomas Gerholm has presented a paper that is simultaneously historical and contemporary. His broad anthropological treatment of the Jewish yeshiva in the traditional Eastern European town and the Islamic madrasa in traditional Morocco, as educational institutions, provides a broad comparative framework in which we stand back and look at modern higher education-in-society. We see just how revealing comparative study of major traditions, e.g., that of the West and Islam, can be. The "idea of the university" as a separate body of masters or fellows took root in one, but not the other.

Islamic colleges remained closely bounded by larger religious structures and tenets. What was not possible in Islamic societies became central in advanced study in Western societies--that the university would develop as a form capable of considerable expansion and absorption, as a central place for enormous specialization. Such broad comparisons, insightfully pursued, surely instruct us about fundamental properties of higher education as well as about the broadest flows of societal development that allow us to live in "worlds of our own making."

Conclusion

I would like to conclude on three basic issues raised by Katrin Fridjonsdottir in her paper. The first is the matter of the "purpose" of organized knowledge. My approach insists that we go ask the actors, and take it from there. Physicists in a physics department can tell us in reasonably clear language what they think they are up to; as can economists; as can historians. Out of their mouths come the purposes of the individual disciplines as on-going concerns, and, more largely, the purposes of the Republic of Science and the academic profession. But when we try to force such academics to specify "the purpose" of higher education, or of organized knowledge, they bog down, as we all do, in a mushy rhetoric that is best left for commencement day and the other ceremonial occasions when we doze in the sun and let the rector ramble on. Higher education is a vast conglomerate of diverse purposes. Pretenses to the contrary are just that--pretenses that something serious has been said when we specify "teaching, research, and service," or "education for democracy," "personal development," and "meeting national manpower needs," or .pa any of the other rhetorical combinations found in national reports in your country and mine.

A second point is whether modern systems of higher education converge. They surely do in such gross characteristics as increased rates of participation, greater differentiation, and governmental concern over expanded costs. But the academic structures and cultures that historical development deposit on the doorstep of current action cause systems to react differently to broad trends. For example, there is now ample reason to think that European systems of higher education are not, en masse, going to converge on the universal, open-door admission of the American system. The secondary-school systems are much different; the national definitions of qualification to enter higher education are quite different. Similarly, despite the strength internationally of the German, British, and American models of integrating basic research and higher education, a host of developing societies may "decide" that research and education are better operated in two distinct spheres of "academy" and "university." In short, there is no reason to use "the higher education system" to mean one type of system. Political systems, economic systems, education systems--all are illuminated by seeking different sub-types and comparing them.

Lastly is the concept of interest that I tried to use as an organizing theme in System. I wanted to use the term in a broad sense, to mean something more inclusive than narrow, selfish self-interest. But as Fridjonsdottir pointed out, the meaning of "interest" then becomes ambiguous. Here, Jane J. Mansbridge's recent formulation is helpful.(16) Mansbridge distinguishes three common forms of interest: "self-regarding"--the form normally meant; "other-regarding," where we commit ourselves to the welfare of our family, our association, our disciplinary peers; and "ideal-regarding," where we commit ourselves to advance the cause of medicine, education, ethical business, sci-

ence, or democracy. Professions clearly attempt to hook self-regard to other- and ideal-regarding interests. Individuals then have a calling, twice over, a life-long absorbing errand richly endowed with symbolic import. In this multi-pronged meaning of interest, we perhaps find a formulation that can usefully orient how we think about academic actors and how we conceive of the imperatives and constraints embedded in their immediate social systems.

NOTES

1) Burton R. Clark, The Higher Education System. Berkeley, Los Angeles, London: University of California Press, 1983 (Paperback edition, 1986).

2) References to the work of the above scholars, and many other scholars in these and other countries, can be found in System.

3) Burton R. Clark, (ed.), The Academic Profession: National, Disciplinary, and Institutional Settings. Berkeley, Los Angeles, London: University of California Press, 1987, (forthcoming).

4) Howard R. Bowen and Jack H. Schuster, American Professors: A National Resource Imperiled. New York: Oxford University Press, 1986.

5) Burton R. Clark, Different Worlds: Academic Life in America. Princeton: Carnegie Foundation for the Advancement of Teaching, 1987 (forthcoming).

6) Walter P. Metzger, "The Academic Profession in the United States," in Burton R. Clark (ed.), The Academic Profession, (forthcoming).

7) Diana Crane, "Cultural Differentiation, Cultural Integration, and Social Control," in Jack P. Gibbs (ed.), Social Control: Views from the Social Sciences. Beverly Hills, California: Sage Publications, 1982, p. 239.

8) Donald T. Campbell, "Ethnocentrism of Disciplines and the Fish-Scale Model of Omniscience," in M. and C. Sherif (eds.), Interdisciplinary Relationship in the Social Sciences. Chicago: Aldine, 1969, pp. 328, 330.

9) Campbell, op. cit., pp. 328, 331.

10) Michael Polanyi, The Tacit Dimension. Garden City, N.Y.: Doubleday, 1967, p. 72.

11) Harold Perkin, "The Historical Perspective," in Burton R. Clark (ed.), Perspectives on Higher Education: Eight Disciplinary and Comparative Views. Berkeley, Los Angeles, London: University of California Press, 1984, pp. 17-55.

12) Chapters by Harold Perkin on the United Kingdom, Wolfgang J. Mommsen on the Federal Republic of Germany, Erhard Friedberg and Christine Musselin on France, and Walter P. Metzger on the United States.

13) Konrad H. Jarausch (ed.), The Transformation of Higher Learning 1860-1930. Chicago: University of Chicago Press, 1983.

14) Alexandra Oleson and Sanborn C. Brown (eds.), The Pursuit of Knowledge in the Early American Republic; Alexandra Oleson and John Voss (eds.), The Organization of Knowledge in Modern America, 1860-1920; and a third volume under preparation. Baltimore: Johns Hopkins University Press, 1976; 1979; and in press.

15) Ralf Dahrendorf, Life Chances. Chicago: University of Chicago Press, 1979, p. 142.

16) Jane J. Mansbridge, Beyond Adversary Democracy. Chicago: University of Chicago Press, 1980.

Appendix

"DISCIPLINARY PERSPECTIVES ON HIGHER EDUCATION AND RESEARCH"
International Seminar, Lidingö, Sweden, September 2-3, 1986

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